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NOTES AND COMMENTS.

Field Observations in Natural History.

SOME time ago (*suprà*, p. 84) we had occasion to lament the meagre character and restricted import of most of the original contributions to Natural History published by the British Field Clubs. The issue of several new reports and journals which we record elsewhere still further justifies this lamentation over wasted opportunities; and the fact that even in so widely-studied a subject as botany no member of the societies affiliated with the Midland Union has done any original work during the last three years worthy of the award of the Union's Darwin Medal, emphasises the need for a further systematic organisation of research. On the former occasion, we pointed out the service that might be done by any widely-gifted naturalist who would produce a handbook stating modern problems in such terms that any ordinary student of nature, dwelling at a distance from towns and libraries, could turn his observations to some profitable account. Quite lately we have had the great gratification of receiving the precise *vade mecum*, which ought to be in the hands of all observers who take an interest in the facts and phenomena of organic nature.

This little work [1] is a new volume of Mr. Murray's University Extension Manuals, and has been prepared by one of the most lucid of modern teachers, Mr. J. Arthur Thomson, of the School of Medicine, Edinburgh. It is a book intended for suggestive guidance of the student, "rather than to satisfy that thirst for knowledge which leads many to intellectual insobriety." The matter is divided into

[1] "The Study of Animal Life." By J. Arthur Thomson, M.A., F.R.S.E. 8vo., pp. 375, with woodcuts. London: John Murray, 1892. Price 5s.

four sections, dealing respectively with the everyday life of animals, their internal activities, their forms and structure, and the general theory of animal life. The style is charming, and the language remarkably simple. The appendix urges the reader to turn his attention to Nature itself, gives a carefully selected list of the special works to use, shows him how to commence and proceed. "Begin with domesticated animals and their history," says Mr. Thomson, "enjoy your seaside holiday, . . . form an aquarium, . . . begin a naturalist's year-book, . . . observe the animals you see on your country walks, . . . work out some subject which attracts you." Let the observer thoroughly understand the principles and spirit of investigation expounded in the four sections of the work; and then he will be prepared to follow these injunctions both with pleasure and profit.

Whether employed as the basis of University Extension Lectures or independently read, Mr. Thomson's "Study of Animal Life" ought to inaugurate a new era in Field Natural History in this country; and we hope it will not be long before we are able to chronicle a sensible improvement in the too frequently aimless and pointless "observations" of the local student of the phenomena of living nature.

The Dispersal of Plants and Animals.

While simple, comprehensive Handbooks and University Extension Lectures may tend to remove the reproach to which we have just alluded, concrete examples of the valuable contributions to the Philosophy of Biology within the power of ordinary Field Observers, are perhaps more to the point. It is thus with great satisfaction that we are able to refer to some recent publications by two recognised leaders in such research. This year's Presidential Address by the Rev. Leonard Blomefield to the Bath Natural History and Antiquarian Field Club, lately issued, is worthy the close attention of all botanists who would do something more philosophical than merely record the geographical distribution of the various plants in their district. A paper contributed by Mr. Clement Reid to the new number of the *Transactions of the Norfolk Naturalists' Society*, is still more suggestive both to botanists and zoologists. Mr. Reid, indeed, may almost be said to inaugurate a new line of research, and this will probably lead to interesting and striking results. He treats of the Natural History of isolated ponds, confining himself to those of artificial origin, usually of quite recent date; and it is astonishing to find how rapidly these small pools of water become stocked with a varied fauna and flora. Certain aquatic animals and plants seem to be transported with the greatest ease; and the problem is to determine, not only the mode and means of dispersal, but also the effect of changed environment on the organisms concerned. So far as Mr. Reid's observations have extended, it appears that the main

transporting agency is that afforded by the feet and feathers of wading and bathing birds; for the commonest plants in isolated ponds are the brittle species with finely-cut leaves, such as collapse and cling when lifted out of the water. The associated molluscs are usually the species that live among, or attach their eggs to, these plants.

With regard to the changes produced by environment, we may also allude to some remarks by Mr. Robert Holland in the August number of *Nature Notes* (vol. iii., pp. 147–152) which have reference to similar observations previously published by Mr. Collingwood Hope. It appears, as might be expected, that fishes placed in stagnant ponds soon become stunted, rarely growing to their normal size, but it is interesting to have proved that even after several generations, this dwarfing in the case of tench is not permanent, the small fishes attaining their usual dimensions as soon as they are removed to a shallow pond with a plentiful supply of fresh water.

Economic Science.

Such observations, of course, have at present merely a philosophical interest, and some of the lukewarmness in this line of research may be due to the preference of many workers for subjects that have an immediately economic bearing. In this connection it is of interest to refer to the exhortation lately delivered by Professor Herbert McLeod to one of the most economically-minded bodies in the world of science—that of the chemists. In his recent Presidential Address to the Chemical Section of the British Association, the Professor refers to Faraday's well-known production of benzene from oil gas in 1825, when this investigator had not the slightest idea that it could ever have any practical application; and he then quotes from Professor A. W. von Hofmann's lecture on Mauve and Magenta in 1862, when the purely scientific discovery had become the basis of a great industry. He quotes von Hofmann's peroration, and it will bear reprinting again:—

"Need I say any more? The moral of Mauve and Magenta is transparent enough; I read it in your eyes. We understand each other. Whenever in future one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly-discovered compounds, you will not cool his noble ardour by asking him that most terrible of all questions, 'What is its use? Will your compound bleach or dye? Will it shave? May it be used as a substitute for leather?' Let him quietly go on with his work. The dye, the lather, the leather will make their appearance in due time. Let him, I repeat, perform his task. Let him indulge in the pursuit of truth—of truth pure and simple—of truth not for the sake of Mauve, not for the sake of Magenta, let him pursue truth for the sake of truth."

"This," remarks Professor McLeod, "seems to be the true spirit of the scientific investigator"; and when as much is known of the fundamental principles of life as is already known about the Atomic and

Molecular Constitution of bodies, Chemical Affinity, and the Conservation of Energy, there is little doubt that this knowledge will have as important a bearing upon everyday affairs as the great industries that have been founded on our knowledge of non-living matter.

Colour Blindness.

Biologists are already called upon to give advice in many matters of ordinary life, and among the latest subjects submitted for their consideration is that of colour vision. A Committee of the Royal Society of London, composed both of Biologists and Physicists, has presented a report on the subject to Parliament, and this is now issued as a Blue-book. It appears that nearly four per cent. of the males in civilised countries exhibit some defect in colour-perception; and most of these persons are practically blind to one of the primary colours of the spectrum. Blindness to red appears to be most frequent, while blindness to green is not uncommon, and there is rarely failure to perceive blue. Some people are blind to two colours, and instances are known in which no difference could be recognised between the three primary colours. The industrial importance of such colour-blindness, in relation to marine and railway signalling, is too apparent to require comment; and the Royal Society's Committee will have done good service if their Report leads to satisfactory legislation.

The Skeleton of the Voice-Organs.

Among the most interesting of recent researches in the development of the animal organism, are those devoted to the elucidation of remnants of embryonic structures in the adult. The preliminary results of these researches sometimes prove to be rash speculations, and we are occasionally inclined to question the value of such journals as the *Zoologischer Anzeiger* and the *Anatomischer Anzeiger*, in which they chiefly appear. In the last-named journal of August 27, however, there is a noteworthy publication by Dr. Harris G. Wilder on the cartilages of the larynx in the Amphibia, in which some striking, novel views are advanced for examination. It is well-known that at least four of the pairs of bars that support the gills of the young tadpole become fused together to form the hyoid plate supporting the tongue of the adult frog. The question now arises as to what happens to the fifth (or hinder) pair of gill-arches when the time arrives for their disappearance or modification. Dr. Wilder believes his researches will show that these elements are transferred to the voice-apparatus, and become the arytænoid cartilages of the larynx. Du Bois has already determined that the thyroid cartilage of the larynx is derived from part of the second and third gill-arches; and Dr. Wilder's new view is further rendered probable by the fact that no animal possesses both the fifth gill-arch and the arytænoid cartilages, exhibiting only one or the other supplied by one and the same nerve.

Hairs and Feathers.

Another remarkable speculation that also necessitates further independent research, appears in the current number of the *Morphologisches Jahrbuch* (vol. xviii., pp. 717–804, pls. xxiv.–xxvi.). As is well-known, it is the common belief that the hairs of mammals, the feathers of birds, and the scales of reptiles are all epidermal structures of a fundamentally identical character; but after an elaborate study of the growth and development of these several protective coverings, Dr. F. Maurer, of Heidelberg, now arrives at the conclusion that hairs are, in every respect, distinct from feathers and reptilian scales. He considers that they are homologous with the sensory points in the skin of the Amphibia, or, at least, that they are outgrowths from these points as bases. Referring to the fact that the characters of the integument are of importance in classifying the great groups of Vertebrata, Dr. Maurer thus concludes that his researches confirm the supposition that the Mammalia are derived directly from the Amphibia, and have not had any Reptilian ancestors.

The Dawn of Life.

Among recent discoveries in the rise and progress of the world of life as a whole, naturalists will turn with perhaps most interest to Dr. Charles Barrois' announcement of the discovery of Radiolaria in the Archæan rocks of Brittany (*Comptes Rendus*, vol. cxv., pp. 326–328, 1892). The so-called *Eozoon* proving to be most unsatisfactory, another claimant to represent Pre-cambrian Life will be welcomed by those who still hope to decipher the conditions of that remote period; and the evidence has been discovered under circumstances that have long been thought to betoken organic agency. The fossils occur in a fine siliceous matrix (phtanite) associated with fragments of graphite, in the neighbourhood of Lamballe, Côtes-du-Nord. They have been submitted by Dr. Barrois to M. Cayeux, who regards them all as simple Radiolaria, of the family Monosphæridæ; and a detailed description is promised for an early date.

The Chemist as a Stratigraphical Geologist.

While referring to progress in the study of fossils in France, we must not omit to mention a remarkable—some might say fantastic—conclusion recently arrived at by a chemist, M. Adolphe Carnot, in the study of fossil bones by chemical analysis (*Comptes Rendus*, vol. cxv., pp. 337–339). This author believes, in fact, that the relative age of fossil bones can be ascertained by estimating the amount of fluoride of calcium entering into their percentage composition. While admitting that these fossils vary much in composition with the nature of the matrix, M. Carnot concludes that "there exists a sufficiently constant relation between the amounts of fluoride and phosphate of

lime contained in fossil bones of the primary and secondary epochs. There is much less fluoride in those of the tertiary epoch, of the quarternary, and especially of modern times." He further applies his method to determining whether certain human bones found with those of extinct animals are really contemporaneous or are of later origin, and have been introduced into the deposits where they now occur. A human tibia discovered by M. Rivière in association with bones of animals was specially submitted to M. Carnot for examination; and analysis proved that this tibia contained only the same percentage of fluoride as a recent human bone, while the associated animal remains yielded seven to nine times as much fluoride as their recent analogues. Hence, concludes M. Carnot, the human tibia in question belongs to a later period than the other fossils with which it was found—a result in which M. Rivière acquiesces on other grounds.

The Miocene Mammals of France.

It is not a little remarkable that the wonderful discoveries of Tertiary mammals made during the last twenty years in the United States have been paralleled by a great advance during the same period in our knowledge of the fossil mammals of Europe, and more especially France. As our readers may be aware, it is within that space of time that we have become acquainted with the marvellous fauna of the Phosphorite beds of Central France through the labours of Dr. H. Filhol, while our knowledge of the mammals of the Oligocene beds of St. Gérand-le-Puy, and of the Miocene of Sansan in the Gers, has been greatly augmented. It is true, indeed, that the extinct European mammals of these horizons do not include many of the strange forms characterising the equivalent beds on the other side of the Atlantic; but they comprise, on the other hand, many types which are quite unknown there, and their importance to the palæontologist and evolutionist can scarcely be overestimated.

Among the faunas of which our knowledge has advanced with such rapid strides during the last few years is that of the village of La Grive-St.-Alban, in the Isère, belonging to the upper division of the Miocene period. The mammals of this fauna were first brought to notice by Dr. Jourdan in the year 1861, and new forms from the same beds were subsequently described by MM. Filhol, Chantre, and Depéret; but it has been reserved for the writer last mentioned to make us acquainted with the full extent and importance of the fauna in question. This task has been completed (so far as present materials permit) in a memoir just published by Dr. C. Depéret,[1] which treats not only of the mammals of La Grive, but likewise of those from other *gisements* in the Rhone basin; and when we mention

[1] "La Faune de Mammifères Miocènes de La Grive-St.-Alban (Isère) et de quelques autres Localités du Bassin du Rhone." *Archiv. Mus. Lyon*, vol. v., pp. 1–94, pls. i.–iv.

that this memoir has been published in the *Archives* of the Lyon Museum, it goes without saying that, both as regards letterpress and illustrations, it is all that can be desired.

From the La Grive deposits, Dr. Depéret now recognises upwards of 46 species of mammals, the great majority of which are identical with those of Sansan. Indeed, most of the species peculiar to the Isère locality are mammals of small size, the remains of which are naturally liable to escape detection. The most interesting part of the memoir before us relates to the peculiar creature known as *Macrotherium*, and it is to this portion that our few remarks will, in the main, apply.

It may be within the knowledge of many of our readers that the Miocene Mammal known as *Macrotherium* was first described on the evidence of claw-bones of the feet, which apparently indicated an animal more or less closely allied to Edentates, like the living Pangolin and the extinct *Megatherium;* and that, although other bones of the limbs had been obtained, its skull appeared to be still unknown. On the other hand, the mammal which had been previously named *Chalicotherium* was known merely by the skull and teeth, which resembled those of certain Perissodactyle Ungulates. Now, only a few years ago, Dr. Filhol startled the zoological world by the announcement that the presumed Edentate *Macrotherium* and the Ungulate *Chalicotherium* were really one and the same, the limbs of the one and the skull of the other having been found in association at Sansan; and it is not a little remarkable that this discovery should have been so shortly followed by a second at La Grive, where a whole skeleton was likewise brought to light. Unfortunately, however, this specimen was broken up before Dr. Depéret appeared on the spot; and when he arrived it was only possible to rescue the limbs, skull, and a few vertebræ.

The results of the observations of the learned author have been to show that, although *Macrotherium* of the Sansan and La Grive Miocene is closely allied to *Chalicotherium* of the overlying Eppelsheim beds, yet that the two are really distinct, the so-called *Ancylotherium* of the Pikermi beds being identical with the latter. The chief point of difference between the two is to be found in the circumstance that, whereas in *Macrotherium* the fore limb is much longer than the hinder—this being especially shown by the radius being nearly twice the length of the tibia—in *Chalicotherium* the two limbs are approximately equal. There is, moreover, a difference in the structure of the bones of the claws, those of *Chalicotherium* being less deeply cleft than in the other genus; and it is curious to observe that in both these respects the later form is less specialised than the older one.

In regard to their affinities, it may be observed that Dr. Filhol was disposed to regard these remarkable creatures as most nearly allied to the existing Edentates, his opinion being largely based on the circumstance that the skull exhibited the smooth contour in the region

of the brain-case characteristic of these animals. The specimens described by Dr. Depéret show, however, that this absence of ridges on the skull is entirely due to the immaturity of Dr. Filhol's example; and the Edentate theory is, therefore, once for all demolished. On the other hand, from the fact that the molar teeth of *Chalicotherium* and *Macrotherium* are extremely close to those of the extinct Perissodactyle genus *Palæosyops*, while many features in their osteology—especially the transverse elongation of the head of the radius—are likewise of an Ungulate type, Dr. Depéret comes to the conclusion that these creatures were merely Perissodactyle Ungulates specially modified for a fossorial, or possibly an arboreal, life. In this view we are entirely disposed to concur, seeing that all the peculiar features of these animals are just those which might have been produced by adaptation for a particular purpose from an ordinary Perissodactyle Ungulate. Still, however, it must be confessed that to find an Ungulate armed with long curved claws, and furnished with disproportionately elongated fore-limbs, like those of a sloth, comes as somewhat of a shock to our ordinary preconceived ideas of an Ungulate.

The second most interesting point in the memoir before us relates to the gradual approximation to modern Pigs presented by the molars of the later representatives of the extinct Oligocene and Miocene animals known as *Hyotherium*. In the Oligocene representatives of this genus (separated by Dr. Depéret as *Palæochœrus*), the last molar has scarcely any hind talon; but in the different varieties of *H. soemmerringi* of the Miocene the author finds such a gradual increase in the length of this talon as to render it difficult to determine where *Hyotherium* ends and *Sus* begins.

The Quarternary History of the Baltic.

Of late years, so much has been written on the Baltic and its glaciation, that we are glad to welcome a summary of the present state of our knowledge, especially when it is accompanied by a full bibliography.

In a paper of 120 pages, Herr Henr. Munthe deals with the various Quarternary deposits of that region (*Bihang till K. Svenska Vet.-Akad. Handl.*, band xviii., afd. ii., no. 1). Following Professor Nathorst, he divides the Quarternary period into five epochs: Post-glacial, Younger Glacial, Inter-glacial, Older Glacial, and Pre-glacial. Then, taking these epochs in regular order, he describes the deposits and the fauna and flora which characterise each. As regards the so-called "Pre-glacial" strata of the Baltic, we cannot help feeling somewhat doubtful, for at present the list of fossils does not contain a single characteristic "Pre-glacial" species, and it is not clear how the fauna is to be distinguished from that of the "Inter-glacial" deposits. The reindeer and dog (*Canis familiaris*, var. *groenlandica*), both unknown in our Pre-glacial deposits, are the only land mammals of which the

species is given. The elephant and the rhinoceros are recorded, but without specific name. As the common Pre-glacial elephant (*Elephas meridionalis*) and Pre-glacial rhinoceros (*Rhinoceros etruscus*) are quite distinct from the mammoth and the woolly rhinoceros, the determination of these species might help to settle the question.

The Geological Survey of Great Britain.

We are glad to learn from the Report of the Director-General of the Geological Survey that a re-survey of the South Wales coal-field has been commenced on the scale of six inches to the mile. From a practical point of view, no more important work could be undertaken by the Geological Survey, but we fear that some time must elapse before the work is accomplished, as at present only one officer—Mr. Aubrey Strahan—is engaged in the survey of that large area. The original survey was made by De la Beche, Logan, and others, about fifty years ago. As there are other coal-fields, including those of the Forest of Dean, Bristol and Somerset, South Staffordshire, &c., that should be surveyed geologically on the six-inch scale, it would seem necessary to increase the staff if the work is to be carried out during the lifetime of anyone now living.

The survey of the north-west Highlands has been vigorously prosecuted, and the Director-General draws attention to the important scientific results that follow from the determination of the Pre-cambrian age of the Torridon Sandstone. The aid of photography has been appropriately introduced to illustrate the structure of the Archæan Gneiss. Another important piece of work is the determination that the "Calcareous Sericite-schists," which extend as a traceable band through the whole of Perthshire into Forfarshire, are a continuation, in a more metamorphosed condition, of the shales or slates of Ardrishaig.

Archæology in Dorsetshire and Wiltshire.

We have just had the opportunity of seeing the large quarto volume, entitled "Excavations in Bokerly and Wansdyke, Dorset and Wilts," by Lieutenant-General Pitt-Rivers, F.R.S. This forms the third volume of a series of researches which he has been carrying on.

In the two former volumes General Pitt-Rivers described the excavations he had made during a period of ten years in the neighbourhood of Rushmore, in Wiltshire. The particular tract is a part of Cranbourne Chase, that lies along the Wiltshire and Dorsetshire Downs, south-east of Shaftesbury. Evidence was obtained of two Romano-British villages, just outside the park of Rushmore; both villages were alike in their general arrangement, and their chief feature consisted of pits, 3 feet 6 inches to 10 feet in diameter, and 3 feet 6 inches to 9 feet deep. The pits were filled with earth and

refuse, so that no trace of them could be seen at the surface, but upwards of 90 were found in each village, and in both cases they were drained by deep trenches. The excavations showed that one of the chief concerns of the inhabitants in those days, was to carry off the heavy rain, of the prevalence of which there is historical and other evidence. Thus a well 188 feet deep was re-excavated, and the Roman bucket found at the bottom, but no water, suggesting that the water-line or ordinary plane of saturation must have lain somewhat higher in the hill in former days than it does at present. The pits were probably used to contain refuse, and they were subsequently used as places of burial.

Among the animal remains, those of horses, oxen, and sheep were of small size, the horse rarely exceeding the size of the Exmoor pony, while the sheep were of a breed the like of which is only to be found at present in the island of St. Kilda. The pig, but slightly removed from the wild boar, was of large size, and the dog varied from the size of a mastiff to that of a terrier. The people themselves were of small stature.

Of the many interesting remains there were a number of iron styli, which showed that the people were able to read and write; and one decorated tablet of Kimeridge shale appeared to be of the kind used for writing upon with the stylus, by means of a coating of wax spread over the surface.

During the past three years (1888–1891) General Pitt-Rivers has turned his attention to the Bokerly Dyke and Wansdyke, and the evidence upon which the date of the latter has been to some extent determined, has been derived chiefly from the careful record of discoveries made in the two Romano-British villages before described. Both earthworks, at the places where excavated, are Roman or post-Roman, and the theory of the Belgic age of the Wansdyke has been completely overturned.

Bokerly Dyke, the present boundary-line between Dorset and Wilts, is an entrenchment of high relief, nearly four miles in length, running in a north-west and south-east direction, across the old Roman road, which runs from Sarum to Badbury.

The well-known Wansdyke runs, or probably did run, from the fenny country in the neighbourhood of the Severn at Portishead, by Bath, passing to the north of Devizes into Savernake Forest, and on to Chisbury Camp, where it turns and runs southward in the direction of Andover.

Full accounts of the excavations are given, and also of the Pottery, Coins, and implements of Bronze, Iron, and Bone that have been obtained. There are notes on the Human Remains from Woodyates on the Bokerly Dyke, by Dr. J. G. Garson; and notes, for comparison, on human skulls from Hunsbury Camp, Northampton, and from the Roman Villa at Llantwit Major near Cardiff. Numerous plates serve to illustrate the characters of these remains and of the other objects

obtained. In fact the work is full of valuable material of interest to the Antiquary and to the student of Prehistoric Archæology, where Geology may be said to merge into History. Referring to the detailed nature of his work, and especially to fragments of pottery, the author says that "tedious as it may appear to some, to dwell on the discovery of odds and ends, that have, no doubt, been thrown away by their owners as rubbish, ... yet it is by the study of such trivial details that Archæology is mainly dependent for determining the dates of earthworks."

Although General Pitt-Rivers holds the appointment of Government Inspector of Ancient Monuments in Britain, the excavations have been conducted entirely at his own expense. In Germany extensive explorations of a similar nature have been carried out by the Government of that country; but, as the author remarks, "it is, perhaps, more in harmony with the recognised custom of our country to leave such works for private enterprise." Fortunately, General Pitt-Rivers has had the three most necessary requirements for original research —time, money, and ability. He acknowledges also the aid of his private staff of four assistants.

The volumes are printed privately, being intended for workers. The author believes there would be no demand on the part of the public for a work of so much detail; for "few persons, even among those who attend archæological meetings, put themselves to the trouble of checking opinions, by sifting the evidence upon which they are based."

The specimens have been placed in a museum erected by the author at Farnham, in Dorset. He is, however, of opinion that, so far as the general public is concerned, museums must be supplemented by other inducements to make them attractive. Within a short distance of his museum he has formed a recreation ground, where his private band plays every Sunday evening in the summer. This ground was attended last year by 16,839 persons, and nearly half of the number visited the museum. General Pitt-Rivers has likewise built a Museum Hotel, so that visitors can find first-class accommodation. He thinks it a mistake to suppose that country towns are the best localities for local museums. He finds the attendance at the Dorchester Museum only 2,826 as against 7,000 at the Farnham Museum; and no doubt the outing is in itself an important accessory in a visit to a country museum, for it draws people out of the towns.

Indexes of Science.

We are glad to see the discussion now in progress in *Nature* about the recording of Zoological literature. It is an old story, and suggestions are many; but those willing to devote the necessary time to recording are few and far between. That something must be done is evident, for over and over again we meet with papers in which

strange ignorance of previous work is patent; and this is not to be wondered at, for did not Sir William Flower, in his recent book on the horse, tell us that more than two thousand papers had been written on that animal alone? We are not surprised, therefore, that authors avoid, or ignore, such masses of literature—life is not long enough as it is, and we cannot spend the whole of it in finding out whether a previous author has counted the spots on a beetle's wing-case, or labelled the hairs on a caterpillar. The suggestion made by one of the correspondents in our contemporary, that there should be a central agency—a Romeike, in fact, for Natural History—is an excellent one; but who is to carry the project into execution? For such an agency must be complete and perfect, else one is no better off than before. There are too many *attempts* at indexes and bibliographies; what is wanted are not attempts, but completed efforts. Such works should be undertaken by the larger institutions, and undertaken on the distinct knowledge that they will be a financial loss. The lamentable indifference of the scientific public, and even of the institutions themselves, is always felt by the compiler or the publisher of one of these time-saving books of reference. The Naturalist will not buy a book; rather than spend his few shillings, he will suffer any amount of inconvenience, and "wait till he goes to the library next time" to see it. The societies grant small financial help to bibliographic work, and when they actually undertake the publication of records, these are often done in such a careless manner as to be almost useless. We have in mind several dreary examples of works which should be of infinite value, but which are so imperfect or unreliable as to be more trouble than relief. In short, the question of providing records for the use of working naturalists has yet to be solved, and the sooner the leading academies of the world take counsel together and arrive at a mutual understanding, the better will philosophy be advanced.

The Nomenclature of Insects.

From the general question of recording, we may turn to the difficulties in the nomenclature of insects. The number of brief notes and papers in which "new species" of insects are described is appalling. We place "new species" in inverted commas advisedly, for an intimate acquaintance with entomological literature impresses us with the belief that it is absolutely impossible for the authors who write on many of the groups to have read a tithe of the papers previously published on the subject. Of men we know, their very years make it impossible, even had they read from their cradles onwards. Moreover, a large majority of these so-called "new species" are shortly described, but *not* figured. Now, it is not reasonable to imagine that any specialist can read, say, one hundred descriptions of butterflies, closely allied, and keep a clear idea from mere text of the minute differences insisted upon. It ought not to be allowable to

publish a description of any animal without some figure, either of the animal itself or of its special feature; and the more so would we urge this point in reference to insects, since Dr. Riley has almost paralysed us with the announcement that there are something like 10,000,000 insects altogether. At present, we can only be thankful that they have not all been discovered, and in their discovery we can well afford to "go slow." It is, of course, essential to describe new varieties or "species" when they occur, but surely what is wanted in entomology is more general monographing, and fewer scrappy papers on two or three disconnected forms.

The Grasses of our Pastures.

The recently-published *Annual Journal of the Bath and West and Southern Counties Society* contains an interesting paper with the above title, by Mr. W. Carruthers, who has in several ways lately contributed towards the diffusion of useful knowledge among those practically interested in agriculture, notably by the preparation of the unique set of wheat diagrams noticed in Natural Science for June. In the present paper the author gives the results of a personal examination "of many of the best pastures of England," and also describes and criticises the observations and views of others.

However rich a natural pasture may be, it will always contain plants of no value as food, which are left untouched by animals when feeding; hence a natural pasture is always capable of improvement by the removal of such useless plants. Grasses form the principal bulk of all pastures. Our flora boasts 107 kinds of grasses, all of which are not available or of equal value for pastures. In fact, when we have deducted those which only occur high up on Scotch mountains, above the limits of cultivation, those found in cornfields, on dry walls, in sandy dunes, the water-grasses, those which are very rare, and those undesirable in pastures, we have left only 47 species which may be regarded as the pasture-grasses of England, any one or all of which, if eaten by stock, would be nutritious.

The feeding value of a grass differs greatly at the different stages of its life; in proof of this a table is given based on data contained in a paper by Mr. David Wilson. Here we have a record of the comparative food-value of twelve grasses at different stages, from which it appears that when in full leaf a grass is worth nearly twice as much as when the seeds are ripe. An amount of cocksfoot, sufficient when in the earlier stage for seventy animals, would, when beginning to bloom, only satisfy fifty, after bloom forty-four, and when the seeds are ripe, only thirty-five. The plant in full leaf is vigorously growing and full of plastic food material, which, as the flowering period is reached, becomes worked up into insoluble fibre, or expended to supply energy for the exhausting process of flowering and subsequent production of fruit. Hence the gradual decrease in feeding-value with increasing maturity.

A second table gives the composition of 100 parts of meadow fescue and cocksfoot at different periods. It shows that the amount of digestible albumenoid in the full-leaf stage is about six times as great as in the seeding stage, and also that the most economical proportion of digestible food—one of albumenoids to about five to seven of carbohydrates—is found only in the leaf stage. It follows, therefore, that "the most nutritious pastures are those in which the plants are not allowed to flower and seed, but are kept in leaf by continual grazing of stock," and on this point botanists, chemists, and practical graziers are all agreed.

Apart from the feeding value of a grass, its palatableness must be considered. Animals, as in the case of man, have their likes and dislikes, and a grass, however nutritious when the only available food, becomes useless in a pasture where it is passed over and left untouched. Hence it is important to know what plants are selected and what are rejected by grazing stock. The identification of the former is not an easy matter, as favourite grasses are so much eaten down that it is hard to recognise the species. But, as the author suggests, by enclosing a few square yards of grass land that has been thus eaten down, the stock will be kept off, and the various species will grow up and flower, and can then be easily identified.

As regards rejection of plants by stock, Mr. Carruthers suggests a very simple method of determination. It is merely to note those grasses which have been allowed to run to seed, and have, therefore, obviously been passed over, as in eating the leaves cattle will also bite off the top of the undeveloped stem, and thus prevent flowering. By this means, the farmer will ascertain not only what grasses he is to reject from mixtures for new pastures, but also what weeds should be got rid of; for the latter, though perfectly innocent in themselves, are occupying the space and consuming the food which would otherwise be at the service of nutritious plants. Yarrow and ragwort are both found in many pastures; the former is palatable to the stock, and is everywhere eaten down, but every plant of ragwort runs up to flower, thereby proving its unpalatable nature.

A short account is given of the "Rye-grass Controversy." Scientific agriculturists are divided in their opinions as to the value of this grass. Mr. De Laune observed that his rams passed it by when selecting food in a mixed pasture, and also attributed the failure of new pastures after three or four years to the fact that rye-grass, which formed the principal bulk of the mixtures employed, is a short-lived grass. Dr. Fream, on the contrary, adduces the universal presence of rye-grass in old grass lands, frequently forming a large percentage of the whole, as evidence of its value, and also asserts that its persistence in an old pasture proves it to be perennial. Its presence is, however, no proof of value as a food material, but argues against it, if the grass is short-lived; for then it could only persist if allowed to seed, and this it could not do if eaten down by stock. A

remark of Sinclair's in his *Hortus Gramineus Woburnensis* is also against Dr. Fream, for "rye-grass," he says, "is but a short-lived plant . . . continued by its property of ripening an abundance of seeds." He also draws attention to the fact that the seed-stalks of rye-grass form a large proportion of the bents in an old grass-land.

As regards other grasses, dog's tail, barley-grass, yellow oat-grass, sweet vernal grass, and the different kinds of agrostis cannot be reckoned among the valuable grasses in a meadow. On the other hand, cocksfoot, fox-tail, meadow fescue, tall fescue, and timothy are eaten down so closely as to be scarcely discernible by an ordinary observer, and wherever one or more of these form the bulk of a pasture it has a high character. The meadow grasses may also be classed with these. In the case of grasses that are equally palatable to stock, the amount of nutritious food produced is an important consideration in estimating their value.

The paper concludes with a table of results obtained by Mr. Wilson bearing on this aspect, from which it appears that the feeding value of the produce of a given area in cocksfoot is twice as great as in rye-grass, and more than three times as great as in sheep's fescue.

LAMARCK'S doctrines are still in the ascendant in America, and the latest contribution to philosophy of this kind has reference to the mechanical origin of the scales of fishes (*Proc. Acad. Nat. Sci. Philad.*, 1892, pp. 219–224). Professor J. A. Ryder, in treating of this subject, points out that the scales are primitively arranged in direct relation to the muscular segments of the trunk; and he believes that the earliest scales were rhombic, because the connection between the muscle-plates and the lower layer of the skin is such, that the integument would be thrown into rhombic areolæ during contraction.

AN interesting case of commensalism is recorded by Mr. A. Alcock, in the September number of the *Annals and Magazine of Natural History*. In 1889 two specimens of a small scorpænoid fish (*Minous inermis*) were dredged off the Godávari Delta, on the Coromandel coast, one of which was covered with a fleshy colony of polypes, since determined as new and named *Stylactis minoi*. The same association of fish and polypes was again observed in a haul off the Malabar coast in November, 1891, and also in January this year, when a single specimen of *Minous* was taken between the Deltas of the Ganges and the Máhánadi. Although numerous other fishes were hauled at the same times, only the *Minous* was found to be infested with the polypes. Mr. Alcock inclines to the belief that this investing growth assists the fish to obtain food by giving it a deceitful resemblance to the incrusted rocks of its environment, while the polypes obtain in return a more abundant supply of food than if stationary or independent.

In an earlier number (*suprà*, p. 101) reference was made to the peculiar group of large horned or hornless Ruminants more or less closely allied to the Giraffe, such as the Helladothere of Greece, the Samothere of Samos and Persia, and the Sivathere and Hydaspithere of India. Hitherto this group has been known only from the countries above-named, but we have now to record that a member of the assemblage has been obtained from the Pliocene strata of Oran, in Algeria. For this new creature Mr. Powel proposes the name of the Libythere (*Libytherium*). This new discovery is of the more interest, since it shows that the ancient Pliocene Giraffes of Europe and Asia were accompanied in their migration into Africa by a member of the Helladothere group, although whether the latter ever reached the true Ethiopian region must await further investigation.

Professor Karl Möbius has recently been studying the hair of the existing elephants, and communicated the result to the Royal Prussian Academy of Sciences. He finds that, in addition to the sparse long hairs, there are most distinct traces of a fine under-fur; and this fact is of great interest in connection with the remarkable development of the wool in the extinct mammoth of Siberia.

An account of "Charles Moore, F.G.S., and his work," has been contributed to the *Proceedings of the Bath Natural History and Antiquarian Field Club*, by the Rev. H. H. Winwood. Moore's life was largely devoted to palæontological studies among the Rhætic, Liassic, and Oolitic rocks, more especially of Somersetshire and Glamorganshire. To him, mainly, we owe the recognition of the Rhætic beds in this country. His chief work was, however, among the fossils, and the large collection he gathered together has brought fame to Bath, in whose museum they were placed. Unfortunately, nearly all the specimens have lately been removed from the room in which they were arranged by Moore, into an upper room, the Saurians affixed to the walls remaining below. The memoir is appropriately accompanied by a list of the fossil types and described specimens in the Bath Museum by Mr. Edward Wilson; and we are informed that Mr. A. Smith Woodward has in preparation a monograph of the Fossil Fishes of the Upper Lias, an exceedingly fine series having been obtained by Moore from Ilminster.

At the recent meeting of the British Association, Dr. Schmitz of Greifswald, made an important communication on the tubercles formed in the fronds of certain red seaweeds of the family Florideæ. These he attributed to the action and growth within the tissues of Bacteria—a remarkable discovery in many ways, since the sea must now reckon its Bacteria, and the plant-world this time furnishes another instance of disease caused by this agency. Mr. George Murray criticised Dr.

Schmitz's position with some severity, pointing out the cases of similar malformations described by Miss Barton, caused by copepoda, nematode worms, &c., and further insisting that since no proof of infection was forthcoming, there was no more than a superficial reason for believing that these Bacteria were *propter hoc*—he strongly inclined to regard them as *post hoc*.

THE vines at Pyrgos, on the West Coast of Greece, have been attacked by a disease which has caused great havoc. A letter from Vice-Consul Fauquier, reproduced in the *Kew Bulletin*, states that some localities are literally devastated, others to the extent of 50 to 60 per cent., while those that have suffered less show a damage of 10 to 20 per cent. Unfortunately, the disease shows no sign of abatement; even if it makes no further progress, the damage done in Pyrgos and Olympia probably amounts to a third or half the crop. The Black Corinth or Zante grape-vine, from which the currants of commerce are obtained, is a variety of the common vine, with seedless fruit; it is cultivated chiefly on the mainland of Greece and the Ionian Islands.

THE September number of *Grevillea* is "under new management," namely, the editorship of Mr. Massee and the active co-operation of the proprietor, Mr. Batters. Two plates are novelties, and, though very "processy," are better than nothing. The new management would do well to depend more on readable contributions than on pages of lists and diagnoses. The mantle of Dr. Cooke has indeed descended on Mr. Massee, and it has not proved a misfit. A useful part of the new number is that devoted to critical and bibliographical notices, these being good-humoured and impartial in all cases except one (p. 27). The last would have been intelligible under the old *régime*, but it may be doubted if the new management is acting prudently in taking over the *ill*-will of the business.

THE series of Swiney lectures on Geology, to be delivered at the Natural History Branch of the British Museum this month, is the only official attempt made during the year to popularise the vast and interesting Natural History collections stored in the National Museum. It is, indeed, difficult to comprehend why the authorities should have even omitted to build a lecture theatre when the grand idea of a Museum of Natural History was being realised. In the original plan, as sketched by Sir Richard Owen, this most important feature was not omitted. At present, the lectures are given in the Geological Library, with the result that numbers of people are unable to obtain admission, while the regular staff and students are seriously inconvenienced in their work and researches. The great popularity enjoyed by Dr. Nicholson has, no doubt, much to do with the success of the past two courses.

I.

Biological Theories.

II.—THE EVOLUTION OF HEREDITY.

IN the previous article it was contended that the phenomenon called "heredity" was merely a likeness of effects due to likeness of the causes producing them; the like effects being two similar individuals belonging to different generations of the same species, the like causes being the influences internal and external which have determined the form, structure, and constitution of those two individuals respectively.

The present paper attempts to account for the recurrence, generation after generation, of those "like causes."

In the search for the efficient cause of this recurrence, that is for the cause of heredity, it is necessary to remember and compare the diverse forms in which heredity is exhibited, and at the same time to take due cognisance of the existence and nature of variation.

The following facts will serve as types of the groups of phenomena which it is especially necessary to bear continuously in mind with reference to heredity:—

1. A man not infrequently resembles his father in some features which are comparatively rare in other men.

2. All *Rhabdonemata* closely resemble their grandparents, while they differ so widely from their parents (*Rhabdites*) as to have been referred to a distinct genus. Both *Rhabdonema* and *Rhabditis* are produced sexually.

3. "Soldiers" among one generation of *Termes* are very like "soldiers" of previous generations, and yet quite unlike their parents, and unlike any ancestor whatever, however remote. The occurrence of "soldiers" among the offspring of several species of *Termes*, which geographically are widely separated, appears to point to their occurrence as a phenomenon of very great antiquity.

4. In *Phylloxera* each generation consists of individuals like each other, except in sex, but unlike any individual of the three or four preceding or succeeding generations. In each year there is a cycle of unlike generations, but the individuals of any one generation closely resemble those of the corresponding generations of previous

cycles. Only one generation in each cycle consists of males and females.

5. Hybrids frequently, though not always, exhibit the characters of both parental types blended together. Not infrequently they bear a closer resemblance to a more remote ancestor than to their immediate ancestors (atavism).

6. Though some cells divide, like *Amœba*, into two *like* halves, cell-division not infrequently results in the production of cells which are *unlike* each other, *e.g.*, *Vorticella*, segmenting ovum (after the first two or three divisions).

With reference to Variation, it must be borne in mind that—

1. Slight variation is almost universal.

2. Conspicuous variation, though less widespread, is very marked in certain species and under certain conditions.

3. Conspicuous variation is in some cases more noticeable in the larva than in the adult. This is especially true of some caterpillars.

4. An individual which differs conspicuously from all its near relatives, not infrequently bears a closer resemblance to remote ancestors (atavism).

5. Variation is often very great under domestication and under the influence of a changed environment.

* * * * *

The schools of Pangenesis and of Continuity alike regard heredity as the original and normal course of events, and variation as a secondary and abnormal occurrence brought about by the fortuitous interference of some extraneous influence. Even modern text-books speak of "heredity" and "variability" as if they were concrete entities capable of producing effects. One would laugh at the philosopher who sought to explain the turning of a weather-cock to the south-east on a particular occasion by referring it to the bursting into activity of a long-latent tendency to turn to the south-east. The "tendency to vary" and the "phylogenetic force" of some writers, the "force of heredity" and "force of variability" of others, are equally absurd. T. J. Parker says[1] "*Heredity* . . . is modified by *Variability*," and earlier writers are not less slip-shod in their treatment of these and allied subjects.

It is, however, not so much this blundering between the abstract and the concrete that I wish to emphasise, as the unanimity with which heredity has been regarded as the primitive, and variation as the modified phenomenon. This aspect of current views may be illustrated by comparing heredity to the regular movement of a planet in its orbit, and variation to a deviation of that planet from its orbit through the attraction of some larger body or bodies passing near it.

I propose to turn all this upside down, and looking upon variation as a phenomenon of greater antiquity than heredity, to consider

[1] "Elementary Biology," 1891, p. 145.

by what means it has been limited, by what means the limitation has been progressively narrowed, and by what means the narrow limitation is now maintained. Heredity, in fact, I propose to treat merely as the narrow limitation of variation, and I hope to be able to show that if no such phenomenon as heredity occurred in early stages of evolution, it would of necessity arise by degrees through the action of well-known influences, and without the intervention of any unknown force or gemmule.

At the outset, I am hampered by a difficulty in terminology. "Natural Selection" is a term so familiar in the Darwinian sense, that were it not that I believe a change in the meaning of the term would add to its usefulness, I should not venture to use it at all. One essential factor of "Natural Selection" (in the Darwinian sense) is heredity; and hence natural selection in that sense cannot rightly be invoked to explain the origin of one of its own factors.

Heredity being established, variation being an observed phenomenon, and the struggle for existence being in progress, then "Natural Selection," in the Darwinian sense, will account for the origin of new species. Such is, I believe, essentially the view held by all Darwinians, but I find it necessary, for present purposes, to eliminate the element of heredity entirely from the meaning of the term, and to use the term, so altered, to express the survival of the fittest, even before heredity existed, and independently of heredity. The change in the meaning consists in what is, I believe, called by logicians a limitation of its intension, and consequent enlargement of its extension: it renders the term applicable to a group of phenomena not previously included within its signification, but in no way affects its application to the phenomenon usually suggested by it, except, perhaps, to give it increased accuracy.

It is to Natural Selection in this wider sense, to the survival of the fit before heredity existed, that I now seek to refer the origin of heredity, to the survival of the fittest that I seek to refer the maintenance of those limitations of variation which collectively constitute heredity.

The terms "offspring," "generation," and "parent," for convenience, may be applied even in the case where multiplication is effected solely by fission. I shall speak of the two *Amœbæ* resulting from the fission of a pre-existing one as the "offspring" of that from which they arose, and the latter I shall speak of as the "parent" and as belonging to the previous "generation."

In the earliest stages of the evolution of living things, whatever those stages may have been, there is no necessity, and, it appears to me, no justification for the assumption that heredity existed or occurred. A living mass might well differ in its various parts, and, if so, the separation of two successive portions from one larger parent mass need not necessarily involve the likeness of those separated portions to each other in every detail of structure and constitution,

nor need it involve the likeness of either of them to the parent mass as a whole. Among the separated portions which, by virtue of their constitutions (chemical and physical), were capable of life and growth, there must have been likeness in all those characters which were, under the conditions then existing, necessary to life and growth. Any separated portions not possessing those characters must have died, and need not be further considered.

After many generations all individuals living must have agreed not only in possessing all the characters necessary to life, but also in having descended from an unbroken series of ancestors similarly endowed; and this, not because these characters were of necessity present in all the offspring of all the ancestors, but because all which did not possess them must have failed to live, and therefore to leave descendants. This weeding out of the unfit individuals independently both of heredity and of competition, and the correlated survival of *all* the fit and not of the fittest only, is the first stage in the evolution of heredity. Those stocks fail to increase in numbers whose constitution does not determine the production, by some means or other, of new individuals capable of life, growth and multiplication. Within those limits, however, variation may be unchecked. Similarity in form, size, colour, etc., need not exist among the individuals. A large proportion of the offspring may indeed be incapable of life, but those stocks which produce the largest numbers of new individuals capable of life and multiplication will, by virtue thereof, gradually become most numerous. The first stage in the evolution of heredity is the weeding out of stocks whose variation in certain directions renders them incapable of numerical increase. Let this not be misunderstood; I do not mean that in some stocks there may be a "hereditary tendency to variation" which leads to failure in the struggle for existence; I do not assume that the offspring are from the first like the parents. I have already denied that the division of a cell into two necessitates the likeness of those two; even at this stage of evolution the vast majority of new individuals may be incapable of life. The favoured stocks are those which, by virtue of their constitution, however widely it may vary, are capable of producing an increase in the number of individuals, the variation among the individuals being thus far limited only with respect to those characters which are essential to life.

The second stage in the evolution is marked by the commencement of the struggle for existence in the ordinary sense of the term, that is, the beginning of competition: it is the stage in which each individual comes to be an adverse element in the environment of others. This is the stage when the fit compete against each other, and the fitter alone survive. The competition between individuals is, however, not of immediate interest to us; it is the struggle between stocks, which is the efficient factor in evolution. As individual competition renders certain characters in the individual advantageous to

the individual, it thereby renders the chemical and physical constitution of the stock, upon which the endowment of the individual with those characters depends, advantageous to the stock. The efficiency of the selection depends entirely upon its giving the advantage to those stocks among the new individuals of which the characters advantageous to the individual are numerically most frequent. All stocks may produce some individuals well-fitted and others ill-fitted for competition in various proportions. Those stocks alone succeed in the competition which produce on the average the greatest proportion of well-fitted, the smallest proportion of ill-fitted. That is, the constitution which determines the production of a large proportion of well-fitted new individuals confers an advantage upon the stock possessing it. A constitution which determines undue frequency of variation in a direction disadvantageous to the individual, is not only a disadvantage to the stock, but the disadvantage increases in its efficiency for elimination at every subsequent stage of evolution, till at last it becomes absolutely fatal, and every stock which is endowed with such a constitution is rooted out. Thus is variation in a stock first definitely limited. Stocks not only *differ* but also *vary* in fitness; every unfit stock, every less fit stock, every new stock varying in the direction of less-fitness is weeded out. As the intensity of the struggle increases, this limitation of variation of the stock in unfit directions becomes more and more definite. Thus only those stocks survive in which the constitution is such as to allow of variation only to a very limited extent in certain directions, while in other directions the variation is almost unlimited.

Such a limitation is not, however, what is ordinarily called heredity; it is not a simultaneous limitation of variations in all directions; there is in this supposed stage of evolution none of that thorough similitude among individuals which the continuity school have without justification assumed to have existed *ab initio*. In any one stock or group of stocks in one definite environment variation is, so far, limited in frequency and extent in certain directions only. The range of variation and its frequency is limited only with reference to certain characters; but as the struggle for existence increases in intensity, not only is this average range of variation progressively narrowed, but the limitation is gradually extended to characters hitherto varying almost unchecked. Characters hitherto unstable in the extreme are gradually reduced to stability. The number of characters thus rendered comparatively constant is gradually increased till it reaches near to infinity. The range of their variation is gradually narrowed till it becomes almost infinitesimal both in extent and in frequency. Genera, if not even species, are now established.

This relative constancy introduces a new factor. The whole constitution upon which so large a number of almost constant characters depends, must itself have been reduced to almost complete constancy. It is difficult to imagine that a constitution limited in its variations

so narrowly and in so many directions that none of the innumerable essential characters which depend upon it shall vary appreciably, can yet itself remain variable to any large extent. The variation in the constitution of a stock must, in fact, be extraordinarily slight if no variation shall be discoverable in any of the innumerable characters which that constitution determines in the new individuals. This constancy of stock-constitution, this constancy of the new individual constitutions itself is the guarantee of that constancy in the recurrence of the "like causes" to which I referred in the previous paper. The efficient cause of heredity which we set out to seek is found. It consists in a stock-constitution whose constancy has been gradually brought near to completeness by the indirect effect of natural selection upon it. The struggle for existence has eliminated all stocks varying beyond certain limits. The variation, whatever it may have been, has at every stage been the necessary outcome of the stock-constitution, and the elimination of stocks whose new individuals varied, has been at the same time an elimination of stocks of unfit constitution, that is, of unstable constitution.

Such a stock-constitution having been once established, it is obvious that, except through accident or other disturbing cause, the struggle for existence might well be suspended, even for many generations, and yet no marked variation might appear. On the other hand, a radical change in the environment capable of affecting the constitution *directly* (*i.e.*, not through natural selection) might in one generation blot out the limits of the species. A species whose specific constitution had, through long selection in marine waters, been rendered constant under constant external conditions, might well undergo a sudden or very rapid change on transference to fresh water. Whether the new constitution would be such as to lead to the production of new individuals capable of life and multiplication or not would be, in a sense, a matter of chance. The new individuals, if capable of living, might be unlike the older ones in form, and their new constitution might be such as to lead to wide variation in succeeding generations. If so, a new process of selection through many generations could alone render it again constant, and the new set of constant characters would be determined largely but indirectly by the new environment. A radical change of diet having altered the chemical composition of the circulatory fluid in the body of one of the higher metazoa, it is doubtful whether this would lead to such a change in the tissues nourishing the ova in process of formation as to involve the production of new ova unlike those of former generations; while we know not whether the new ova would be capable of development at all, and are ignorant of the form and structure of the new individuals if the ova did develop. It would thus be as gratuitous on my part to say dogmatically that increased variability in future generations would be the result, as it is for the continuitatists to dogmatically deny that the germ-plasm would be affected. It at

least appears probable that if such variability did arise, a new selection would again limit the variation, and reduce the species again to comparative constancy, though perhaps the new species so formed might differ radically from the old one. That such influences may have helped in the comparatively rapid transformation of species in nature, if not proven, at any rate appears possible.

Whether such *direct* action of disturbing influences actually has had a share in producing the transformation of some domesticated animals or not, it is difficult to determine; but the *indirect* effect of artificial selection is beyond doubt, and the comparative rarity of the "natural" coloration among rats bred in captivity, and apparently not even subjected to artificial selection, lends some support to my view that no character will remain constant for an indefinite number of generations unless that character be the immediate or remote object of some kind of selection. The phenomena described as resulting from "panmixia" are numerous enough, and sufficiently well known to justify me in stating that what my theory would indicate *à priori* is in accordance with observed facts. If heredity be a limitation of variation by the indirect action of selection, then the suspension of that limitation, as a consequence of the suspension of selection—in other words, increased variation under domestication—is comprehensible.

Variation having increased under domestication, a new selection, either natural or artificial, is found, if carried far enough, to produce new limitations of that variation; that is, to render new groups of characters hereditary, groups of characters which, in the case of domestic "breeds," distinguish each breed from others, as well as from wild congeners. It is thus that we have seen new breeds of poultry produced, breeds which have already become sufficiently constant to entitle them to the distinctive names they have received.

This constancy, however, is only relative: even the most constant of domestic breeds is probably less constant than almost any species evolved under the direction of natural selection. The "heredity" is not only not complete, but it is less so than in natural species. The average frequency of variation in the direction of ancestral natural species has been progressively diminished, but has not been reduced to zero. The stocks which most frequently gave rise to animals possessing the desired characters have been selected: those stocks which reproduced the ancestral characters either more markedly or more frequently have been exterminated, and thus both frequency and distinctness of the obnoxious ancestral characters have been reduced: but the characters do nevertheless still occur, and we call the recurrence of them "atavism." It is, in a certain sense, the progressive elimination of atavism by selection which constitutes the formation of a new species or new race.

Though thus offering an explanation of true atavism, I would not for a moment be understood to regard the great bulk of so-called

"atavistic" monstrosities as being of the same nature as the true atavism, shown for instance in the "reversion" of domestic breeds of pigeons to the type of the blue rock pigeon.

Monstrosities like two-toed horses, human beings with "persistent gill-clefts," and the like, belong to a class of pathological phenomena which are, in my opinion, in no way connected, or at most are only remotely connected with true atavism.

That the blending of two constitutions by hybridism should lead to a partial blending of characters distinctive of those two constitutions, is what might perhaps be expected *à priori;* and the exaltation of characters possessed in common by the two constitutions is similarly to be expected, but it is to be remembered that it is stock-constitutions, not individual-constitutions, to which I refer. In the case of pigeons, the characters common to all breeds are those implied by the name "pigeon." All pigeons alike have certain anatomical characters, but there are other characters which distinguish each breed from the others. There are certain constitutional characters common to all domestic breeds; for instance, all appear to be liable to "atavism," though its occurrence is comparatively rare. Experiment has shown (Darwin) that hybrids do possess some of the characters of the separate breeds from which they are derived, and that they are more liable to atavism than pure-bred birds.

Before proceeding to consider some more complex cases of heredity, it is well to summarise the conclusions reached so far. They are the following:—

(1) *Heredity is essentially a limitation of variation, and* (2) *is therefore of less antiquity than variation.* (3) *It is not an absolute limitation; variation still occurs.* (4) *The limitation has been effected by "natural selection" (using that term in a widened sense) operating upon the stock-constitution.* (5) *The limitation has been progressive, the range of variation having been narrowed step by step; the frequency of variation having been diminished step by step; and the number of characters whose variation has been thus affected having been increased step by step.* (6) *The efficient cause of that limitation, that is, "natural selection," being removed, the range and the frequency of variation increase.* (7) *A new selection leads to a new set of "hereditary characters," and this process is similar to that by which "heredity" has arisen.* (8) *The phenomenon of true atavism is simply the occurrence of those characters in a given individual which selection, natural or artificial, rendered less and less frequent, but the frequency of which has not yet been reduced to zero.* (9) *The blending of two stock-constitutions does in fact lead to the phenomena to be expected; notably to a combination of various features characteristic of the two stocks, and an exaltation of at least one characteristic common to the two constitutions,* i.e., *the occasional production of atavistic offspring.*

So far only the more ordinary types of heredity have been considered, those types and varieties which other theorists have sought to explain, that is, those cases in which the production of individuals fitted for successful competition in the struggle for existence, has been

limited to at most two kinds of individuals—males and females: the case of polymorphic species in which some individuals are incapable of taking any part in reproduction has now to be considered. The cases of alternation of sexual with asexual, and of dicœcious with hermaphrodite or parthenogenetic generations will need no special elucidation, for what has already been said and what has now to be said of the Termites, etc., will, with only slight alterations, apply to them also.

In non-social species such as birds, where the society, if it exists at all, includes at most two adults, the males and females alone do all the work, and any other individuals, if produced at all, would not only be useless competitors for food and doomed to die without offspring, but would also be a disadvantage to the stock. More accurately, that stock which, by virtue of its constitution, produced sterile individuals would thereby be at a disadvantage in the *struggle for perpetuation*. Such individuals are occasionally produced, but natural selection weeds out the stocks producing them frequently in the same way as it weeds out all stocks hampered by any other disadvantage whatever: and hence the percentage of "neuters" in *non-social* species is almost infinitesimal.

Among social species it is otherwise. Where all individuals work for the common good, the production of neuters need not be a disadvantage, and it may even be an advantage. The continuous labour of such members, uninterrupted and unhampered by reproductive functions, is of great value to a community; and the stock which, by virtue of its constitution, produces individuals either varying in reproductive capacity, or liable, under certain conditions as to diet, so to vary, is at an advantage as compared with other *social* stocks whose constitution determines the production of fertile offspring only. Hence natural selection will tend, within due limits, to progressively increase the average percentage of neuters (or potential neuters) among the individuals of successive generations. It is *not the individuals* which are selected, but the *stocks*. The unit, corresponding to the individual or a pair in an ordinary species, is in these species the whole society. The stock which is favoured by natural selection is not that producing the fittest individuals, but that producing the fittest society. The constitutional characters which natural selection favours are those upon which the production of the fittest society depends. What society is the fittest depends on the environment of that society in part, but largely also upon the organisation of the society itself. The environment and social organisation of the Termites, or "white ants," are such, that natural selection has determined a stock-constitution, one peculiarity of which is that, instead of the ordinary two types of offspring (male and female), there are six types. That constitution is kept constant by natural selection, and hence those six types of individuals are found among the offspring, if not of every "queen," at least of every society.

It was the facility for explaining this phenomenon offered by the theory I have set forth which first led me, in fact, to believe in my own speculation. Pangenesis always has appeared to me impossible, and "perigenesis" absurd, while "continuity" never appeared to explain the ordinary phenomena, even if such a continuity had been demonstrated to be universal. My own theory appeared merely plausible till consideration of the Termites made it credible.

Hitherto advantageous characters and disadvantageous characters have been referred to, and though I have not explicitly stated that *adult* characters are meant, I fear that, unless I emphasise the opposite of that interpretation, I may be understood to refer to such characters alone. Such an interpretation is by no means intended. When I speak of characters advantageous to the individual, the characters I really mean are the whole series of characters from the very earliest stage in the production of an ovarian cell to the very last moment of life. The favoured stock-constitution is the one which leads to the formation of those new individuals which throughout the whole course of their existence are on the average best fitted for competition at each successive stage. Just as I have insisted on the more or less independent establishment of each character of the individual, so I would now insist on the more or less independent establishment of every character, not only of the adult, but of the individual at every successive stage of development.

There are animals—some *Lepidoptera* for instance—in which the most striking characteristic of the larvæ is their variety, and yet the variation among the adults is so slight as almost to defy detection. If variation in those characters of the stock-constitution which determine the external characters of the larvæ be not disadvantageous to the species, natural selection will have no power to limit that variation, although it may impose the narrowest limitation upon the variation of that same constitution so far as the external characters of the adult are concerned. The only variations in the larvæ which natural selection can limit in extent or in frequency are those which are disadvantageous to the species as a whole.

The form of a caterpillar or of a nauplius is as strictly determined as that of the adult, probably even more strictly. If the efficiency of natural selection is proportional to the intensity of the struggle for existence, then it would seem strange if the stages in which most individuals die should be least affected by that selection. For every adult *Peneus* which dies without contributing to the multiplication of its kind, it is probably no exaggeration to say that a thousand succumb to the pressure of external circumstances in the "nauplius" phase. The characters of the nauplius I hold to be determined in the same way as those of the adult, and the "inheritance" of larval characters I claim to be explicable by the theory I have advanced for the explanation of "heredity" in general.

C. Herbert Hurst.

II.

The History of the Moas, or Extinct Flightless Birds of New Zealand.

SINCE the memorable occasion in 1839 on which Sir Richard Owen received the fragmentary thighbone that led him to infer the former existence of Ostrich-like birds in New Zealand, the unfolding of the history of *Dinornis* and its allies has been a subject of continual interest. The Hon. Walter Mantell's early discovery of the bones of these great Ratite birds mingled with charcoal in the refuse heaps of the Maoris, sustained the hope for a long time that some few survivors might be met with in one of the less known outlying districts of the islands; but subsequent researches have failed to realise the anticipation, and it is now tolerably certain that the race is quite extinct. Under such circumstances, our knowledge of the Moas—as these birds are termed in the Maori traditions—is based solely upon skeletons and the mummified relics occasionally found in fissures and caves. The determination of the genera and species is thus a matter of great difficulty, and no two authors are in precise accord. At a meeting of the Canterbury Philosophical Institute (New Zealand) in October last, Professor F. W. Hutton read a memoir attempting a revision of the whole subject, based upon the fine series of skeletons now preserved in the Colonial collections; and a copy of this interesting work has lately reached us. At the same time the Professor has favoured his European colleagues with the reprint of a newspaper article stating the results of his researches in connection with the more general questions involved. This article has a bearing upon problems of such wide interest that we make no apology for reproducing it in its entirety, before discussing the important advances made in matters of detail.

I.

That the Moas have been a long time in New Zealand is certain. In addition to the immense number of bones found in peat beds and river alluvia of Pleistocene age, remains have been found near Napier, and probably also near Wanganui, which belong to the Newer Pliocene period. The bones of a small species of Moa, found two years ago under a lava stream at Timaru, are still older

and probably Upper Miocene, while the Hon. W. Mantell found in 1849 a fragment of a bone, which probably belonged to a Moa, near Moeraki in beds of Lower Miocene age.[1]

The Ratitæ are generally supposed to have originated in the Northern Hemisphere, and to have spread southwards into Patagonia, South Africa, Australia, and New Zealand. But if so, how could birds which could not fly manage to reach New Zealand without being accompanied by any mammalia? Certainly they did not precede the mammalia, and it is very unlikely that they should twice have swum across straits which were impassable to mammals —once from the Oriental into the Australian region, and again from the Australian region into New Zealand—and there are other reasons for doubting the northern origin of the Australasian Ratitæ. The existing New Zealand Ratitæ (*Apteryx* or Kiwi) are smaller than any of the others, and make a nearer approach to the original flying ancestors; and we should expect to find the smallest and least altered forms near the place of origin. Now there are in Central and South America a group of birds called Tinamus, which, although flying birds, have been shown by the late Professor W. K. Parker to resemble the Australasian Ratitæ in many particulars, and as the connection between South America and New Zealand is well known, it seems more probable that the Moas originated in New Zealand in the Eocene period, from flying birds related to the Tinamus, and that they spread from here into Australia and New Guinea, than that they should have migrated southward from Asia.

In whatever way the Moas originated in New Zealand, it is evident that the land was a favourable one, for they multiplied enormously and spread from one end to the other. Not only was the number of individuals very large, but they belonged to no less than seven genera, containing twenty-five different species, a remarkable fact which is unparalleled in any other part of the world. Africa and Arabia are inhabited by but two or three species of Ostrich; South America, from Peru to Patagonia, has only three species of Rhea; Australia has two species of Emu and one Cassowary; while eight other species of Cassowary inhabit islands from New Britain to Ceram. Outside New Zealand two species of Ratitæ are rarely found living in the same district, while a few hundred years ago there were in New Zealand several different kinds of Kiwi, as well as the twenty-five species of Moas. An explanation of this problem may perhaps be found by examining the present distribution of the Cassowaries. Here we have eight species inhabiting five different islands, and if this region of the earth were to be elevated, and the islands joined together, these eight species would mingle. If the region were to sink once more, all of them would be driven to the highest land, and might be crowded into one small island. Now we know from geology that New Zealand has gone through a series of changes in

[1] May be earlier, according to Capt. Hutton's new memoir.—ED.

level, similar to those just mentioned. In the Miocene period it consisted of a cluster of several islands, which were elevated and united in the Older Pliocene, and ultimately divided into the two islands we have now in the Newer Pliocene. If the ancestors of the Moas inhabited New Zealand during the Eocene period, they must have been separated on these islands during the whole of the Miocene, and mingled together again in the Pliocene. In this way—*i.e.*, by isolation—probably the genera originated, but the species appear to be due to variations without isolation. As is the case with most common animals, the Moas varied greatly, and there being no carnivorous animals to hold them in check, while vegetable food was abundant, natural selection did not come into play, and the intermediate forms were not strictly eliminated. Under such favourable circumstances conditions of life were easy, and the birds got larger and fatter, more sluggish and more stupid. The oldest known Moa is one of the smallest, and it is the smaller species which are found in both islands; from which we may infer that they were the only ones in existence when the two islands were united, and that the Moas since then increased in size. But the very large Moas were always comparatively rare. The commonest kinds in the North Island were only from two-and-a-half to four feet high, while those of the South Island were mostly from four to six feet in height. The giant forms, going up twelve and thirteen feet, were seldom seen.

Throughout the Pliocene period the Moas flourished greatly; but in the Pleistocene they must, in the South Island, have died in large numbers, for how else could such immense quantities of bones have got together in the peat-beds at Glenmark and at Hamilton in Central Otago? It has often been suggested that flocks of birds, attempting to escape from fires, rushed into the swamps and perished. But when we remember that these Moas died thousands of years ago, long before there were any human inhabitants to light fires, it will be seen that this surmise is quite out of the question. Only two hypotheses appear to be possible to account for the facts. Either the birds walked into the swamp and were drowned, or else their dead bodies were washed in. The first hypothesis is probably the explanation of the deposit at Te Aute near Napier, because many of the leg bones were found upright in their natural position. But at Glenmark and at Hamilton the bones were lying in all directions, as often upside down as in any other position, and the peat-beds were only a few feet thick, and filled with bones up to the very top. We cannot, therefore, suppose that these Moas were swamped, and there is evidence in both of these cases to show that the dead bodies of birds were washed in by floods. We find corroborative evidence of this in the alluvial plains of Central Otago, for these always contain numerous bones wherever a stream enters them from the hills.

But how are we to account for the number of dead birds washed down from the hills? There are two remarkable facts connected

with these bone-deposits at Hamilton and Glenmark. One is the very large proportion of bones of young birds from one-half to three-quarters grown; and the other is the absence of Moa egg-shell. These two facts seem to show that the birds perished in the autumn or winter, when the birds of the year were not full grown, and when the females did not contain any hardened eggs. Also, it is evident that dead Moas could not be washed into swamps under the present climatic conditions, and the explanation of the puzzle must lie in the fact that in Pleistocene times, when these bone-deposits were formed, the climate was very different from what it is now. At that time the eccentricity of the earth's orbit was very great, and when winter in the Southern Hemisphere happened in aphelion, long cold winters were followed by short and very hot summers. It seems probable, therefore, that the early winter snows killed large numbers of Moas and other birds on the hills, that their bodies were floated down by summer floods and avalanches caused by the melting snow, and that they were deposited in hollows at the foot of the hills. As the Pleistocene period passed away the climate got more equable and the surviving Moas once more increased and multiplied, until they were ultimately exterminated by the hand of man.

All are now agreed that the Moas were exterminated by the ancestors of the Maoris, and the only question upon which opinion is still divided is, How long was this ago? The case seems to me to stand thus. In the North Island there are several names of places in which the word Moa is incorporated, but in the great number of Maori tales and poems which have been collected by Europeans the allusions to the bird are very slight and obscure, generally, indeed, fabulous. There is also one very ancient poem called "The Lament of Ikaherengatu," in which the phrase "Ka ngaro i te ngaro a te moa" (Lost as the Moa is lost) occurs, which certainly shows that the bird was not in existence when the poem was composed. The so-called traditions of its habits appear to be, in large part at least, late deductions from these words and phrases, and we must conclude that, in the North Island, the Moa was exterminated by the Maoris soon after their arrival in New Zealand; that is, not less than 400 or 500 years ago.

In the South Island there are no names of places containing the word Moa; but here remains have been found—either skeletons lying on the surface or bones with skin and ligaments still attached—which give the impression that the birds were living here not more than ten or twelve years ago. Now the bones which are said to have strewn the surface so abundantly when the first settlers came, had all disappeared in fifteen years; so it is plain that either some change in the surrounding conditions caused the bones to decay, or that none of the bones which were so abundant in 1861 were more than fifteen years old. But as we cannot believe that Moas were abundant in Otago in 1846, we must fall back on the opinion that the fires lighted

by the early settlers to clear the scrub, so altered the conditions under which the bones had been preserved that they soon decayed, in which case we cannot say how long the bones may have been lying there. It is something the same with those bones which still have dried skin and ligaments attached. They are so fresh that, unless the birds lived a few years ago, they must have been preserved under specially favourable conditions; and there are reasons for thinking that the small district of Central Otago, in which alone these remains have been found, is one specially favourable for preserving animal remains. If this be so, we cannot say for how many years they may have been preserved, perhaps for centuries, and as we have every reason to believe, upon the authority of the Rev. J. W. Stack, that the ancestors of the Ngai Tahu, who have inhabited the South Island for the last 200 or 250 years, never had any personal knowledge of the birds, we must allow that the Moa has been extinct for at least that time. On the other hand, it is quite certain that the Moa was exterminated by the Maoris, and the Maoris are not supposed to have inhabited the South Island for more than 500 years, so that the time of extinction must fall between these dates. It seems improbable that the Ngatimamoe, the last remnant of whom inhabited the West Coast sounds a few years ago, were Moa-hunters. The Moa-hunters of the South Island were not cannibals, and as Te-rapuwai and Waitaha, the tribes who preceded the Ngatimamoe, are said to have been peaceful, and to have "covered the land like ants," it lends support to the Maori tradition that it was they who exterminated the Moa, and made the shell-heaps on the beach. If this be so, the Moas were exterminated in the South Island about 300 or 400 years ago; that is, about a hundred years later than in the North Island.

F. W. HUTTON.

II.

Captain Hutton prefaces his new memoir[1] with an excellent *résumé* of the early history of the discovery of their remains. This is followed by a systematic account of the various genera and species; and then we find the interesting discussion as to the origin and extirpation of the Moas already given above.

The author divides the Moas into seven genera, viz.: *Dinornis*, *Palapteryx*, *Anomalopteryx*, *Cela*, *Mesopteryx*, *Syornis*, and *Euryapteryx*;

Skeleton of *Pachyornis elephantopus* (A) and limb of *Dinornis maximus* (B).

the first including the largest, and the fourth the smallest. This classification differs considerably from the arrangement noticed on page 267 of our June issue, although the difference is really less than at first sight appears to be the case. Putting aside the genus *Megalapteryx*, which Captain Hutton does not appear to regard as a Moa at all—although we have no doubt that it is—the forms included by our author under the second, third, fourth, and fifth genera are comprised in the British Museum Catalogue under the head of

[1] The Moas of New Zealand, *Trans. N. Zealand Inst.*, vol. xxiv., pp. 93–172, pls. xv.–xvii. (1892)

Anomalopteryx. *Syornis* represents in part the genus *Emeus* of the "Catalogue"; while *Euryapteryx* corresponds to *Pachyornis* and part of *Emeus* of the latter. The discrepancy in regard to these later genera is due to the circumstance that the skull with a pointed beak referred by Sir R. Owen to the elephant-footed Moa (*Dinornis elephantopus*) belongs, according to Captain Hutton, to the so-called *Dinornis crassus*; while the former species had a broad-billed skull resembling that of *D. gravis*. Assuming this to be correct,[2] *D. crassus* will stand as *Emeus* (*Syornis*); but we have great doubt whether *D. gravis* and *D. elephantopus* of Owen can both be included in *Euryapteryx*, since, according to the restored skeleton sent home by the late Sir J. von Haast, the sternum of the two birds is quite different. We accordingly think that the elephant-footed Moa will probably still have to stand as *Pachyornis*. The following table will show the equivalency of Captain Hutton's scheme, and our provisional amendment of the system followed in the "Catalogue":—

B. M. Catalogue.	*Hutton.*
Dinornis	Dinornis.
Megalapteryx	o
Anomalopteryx	Palapteryx. Anomalopteryx. Cela. Mesornis.
Emeus (crassus)	Syornis (in part).
Euryapteryx (gravis) Pachyornis (elephantopus)	Euryapteryx.

As it will be unnecessary to enter here into the question as to whether it is preferable to use the term *Anomalopteryx* in its narrower or wider sense, we may dismiss the systematic portion of our subject with the observation that we think Captain Hutton has allowed too many so-called species to remain in the typical genus *Dinornis*.

Passing on to the more interesting subject of the origin and extermination of the Moas, the first noteworthy point is Captain Hutton's argument for the derivation of the Moas from flying birds allied to the South American Tinamus, which reached New Zealand during the Eocene period. As we observe from the article printed above, he supports this view by stating that some of the New Zealand Ratitæ are the smallest representatives of that group, and thereby show the nearest affinity to flying birds; while he further adds that there is a difficulty in conceiving how flightless birds could have entered New Zealand without being accompanied by Mammals. We cannot attach very much weight to the former argument, and we cannot but think that when the author admits that the Moas probably passed from New Zealand to Australia during the Pliocene (? Pleistocene) period, without the passage of any mammals in the opposite direction, his arguments as to the impossibility of flightless birds, as such, having reached Australia and New Zealand at an earlier period

[2] We do not at present commit ourselves by saying that it is.

are by no means convincing. Until, then, some evidence of the existence of the presumed Tinamu-like birds in the Eocene of New Zealand be forthcoming, we may be pardoned for continuing to accept Mr. Wallace's view that the Australasian Ratitæ reached their present home when they had already lost the power of flight.

On the other hand, we fully agree with Captain Hutton that, in whatever way they originated, it is evident that when the Moas were once established in New Zealand "the land was a favourable one, for they multiplied enormously and spread from one end to the other." Not only was the number of individuals large, but the multiplicity of genera and species is equally remarkable. When, however, the author further speculates as to the presence of the ancestors of the Moas in New Zealand in the Eocene period, and the occurrence of a different kind on each island of the archipelago then existing, we are loath to accept his views without some evidence from fossil remains. If the genera had been isolated in the way suggested, we should have expected that they would differ from one another more than we find to be the case. Moreover, we have considerable doubts whether several of the genera adopted by our author are really entitled to rank as such.

In regard to Captain Hutton's explanation of the occurrence of an enormous number of Moa-bones found in the peaty swamps of certain districts, we may remark that it appears to us too thorough. A climate which had such disastrous effects on Moa-life would, it seems probable, have finally exterminated the whole breed. Still, however, it may be regarded as certain that the enormous numbers in which Moa-bones occur in some spots must indicate abnormal conditions.

Finally, while admitting the cogency of Captain Hutton's argument for the extirpation of the Moas by the Maories, there may be still some doubt as to his inference from the occasional discovery of remains of the skeleton with skin and ligaments in certain fissures and caves. These remains occur only in a district in Central Otago, where, according to Captain Hutton, climatic conditions are especially favourable for the preservation of animal substances; and in that case it is almost impossible to deduce any conclusions as to their age.

R. LYDEKKER.

III.

The Fore-runners of the Backboned Animals.

IT has long been an interesting problem to determine the nature of the organisms in which first appeared the foundations of a backbone, which gave a new impetus to the evolution of the animal kingdom. Embryology and Comparative Anatomy have been studied in vain, Palæontology has afforded no certain information, and the rival schools of theorists are still in the midst of a conflict, at present without much sure basis in fact. Whether some form of "worm" gave origin to the fore-runners of the great backboned race, or whether a primæval relative of the arachnid "king-crab" turned upside-down and re-arranged limbs and head—these are questions still admitting of endless discussion, no doubt fruitless in their main object, but desirable from the new lines of research they continually suggest. We must, indeed, find the actual ancestry of the vertebrata if we aim at much further progress in the quest; and this is only possible under the most unusual and accidental circumstances, unless it happens that some members of each great section of that ancestry possessed a calcified (or otherwise hardened) skeleton. Much has been learned during recent years of the few skeletons already discovered in the early Palæozoic rocks, where such fossils ought to occur; and as little of this new evidence seems to have hitherto attracted the attention of those most concerned in the study of animal pedigrees, it may be of service to review some of the principal facts. Palæontology cannot yet solve the main difficulties, but it can, at any rate, establish some suggestive points.

With regard to the earliest traces of undoubted Chordate animals in Palæozoic rocks, the United States Geological Survey, nearly two years ago, roused considerable hopes of new evidence from the Lower Silurian (10); but it now appears (11) that these fossils are merely unsatisfactory fragments, and of no value whatever for our present purpose. Equally unsatisfactory, too, are the conical teeth from the Lower Silurian of St. Petersburg, previously ascribed by Rohon to fishes (5). Few will doubt that these fossils belong to Chordata, but beyond that determination no advance can be made.

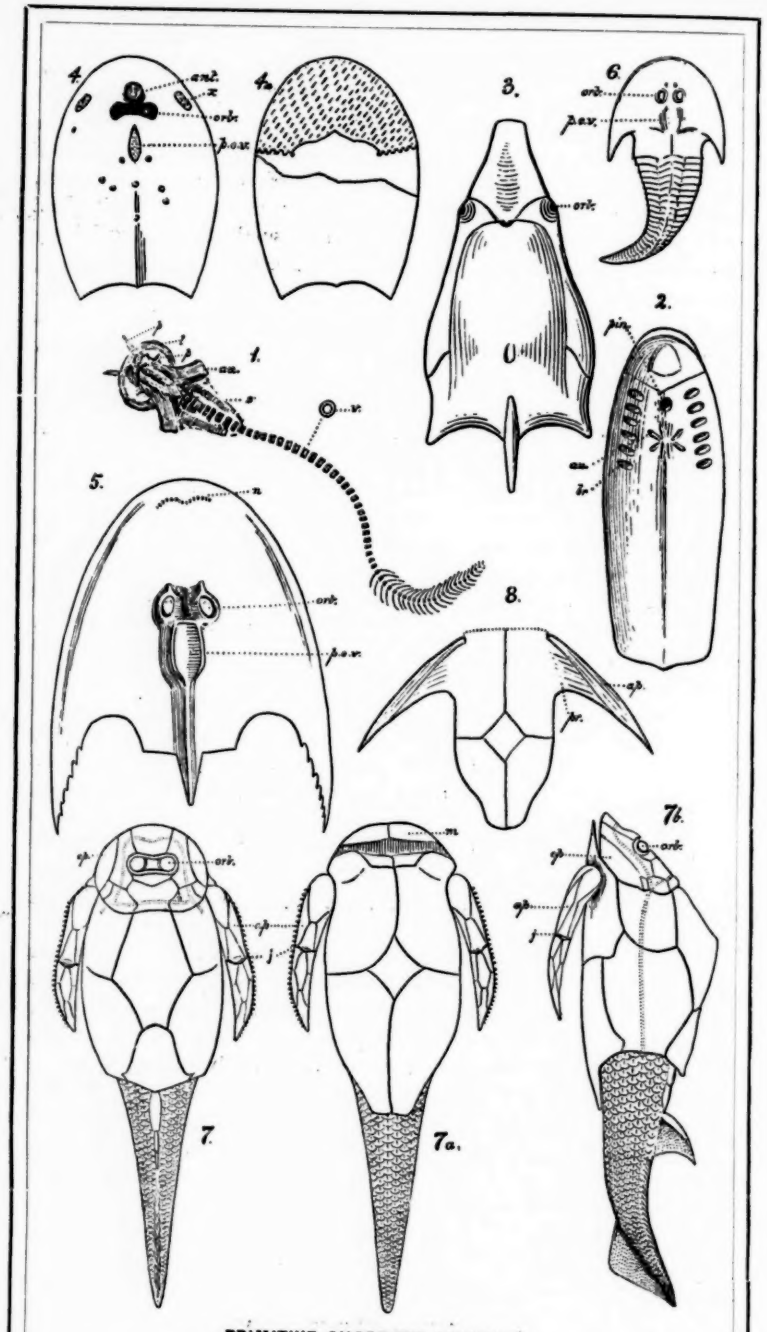
In the case of the remains of Chordate animals from the Upper Silurian and Lower Old Red Sandstone, however, discoveries already made are more satisfactory for consideration; and there are four

principal types of life worthy of special notice. There is a limbless, hard endoskeleton in the Lower Old Red Sandstone; there is a simple shielded organism, with no bony tissue in its armour, in the Upper Silurian and Lower Old Red Sandstone; a similar shielded organism, covered with few plates, but with some bone-tissue in their substance, occurs in the same formations; and a more advanced armoured creature, with a complex of symmetrically arranged bony plates, is met with both in the Upper and Lower Old Red Sandstone, but never earlier.

The limbless skeleton from the Lower Old Red Sandstone of Caithness was briefly noticed two years ago by Dr. R. H. Traquair (9) under the name of *Palæospondylus gunni*; and the writer is indebted to Mr. Donald Calder of Thurso for a small series of specimens from which the accompanying partial restoration (Fig. 1) is made. It belongs to a very small organism, rarely, if ever, exceeding two inches in length. There is a well-formed calcified cartilaginous skull, with a lateral extension on each side at the position of the organ of hearing (*au.*), but with no certain evidence of a pair of nasal capsules; the region of the nose, indeed, is so narrow as to suggest some comparison with the ordinary monorhine lampreys and hag-fishes. In front there is a pair of processes (*p.*), which may be of the same nature as those named prepalatine by W. K. Parker, or may be merely lip-cartilages; a lateral pair of minute points further recalls the lip-cartilages, and a great broad ring (*l.*) crushed upon the anterior part of the skull, bears an extraordinary resemblance to the annular cartilage forming the rim of the mouth in the lamprey. The back of the head is well-defined, and immediately adjoining it posteriorly, there is a hard shield (*s.*) presumably covering part of the back of the animal. The notochord is surrounded by a series of calcified rings (*y.*) developed in its sheath, apparently with short intervening spaces; and towards the end of the tail there are indications of delicate neural and hæmal spines.

Such is the sole information at present available in regard to *Palæospondylus*; and this, it will be observed, merely suffices to place the animal at the base of the backboned series. It *seems* to possess an unpaired nose, lip-cartilages in place of functional jaws, and no paired limbs; thus agreeing precisely with the lampreys and hag-fishes, of which the fossil representatives have long been sought. Theoretically speaking, allies of these limbless Chordata ought to have been a dominant type in the late Silurian and early Old Red Sandstone age; and it is a well-known law that animals, when dominant, attain to a higher degree of development than at any other time in their history. It is extremely probable, therefore, that *Palæospondylus* belongs to this interesting category.

While there is much probability that the skeleton just described can be paralleled among existing animals, there is no possibility of recognising any living congeners of the armour-plated organisms of



PRIMITIVE CHORDATE ANIMALS

which we publish other restored sketches. That the three types, to which reference has been made above, are all closely related and represent three grades in a great extinct group, can scarcely be longer doubted; and it is now customary to arrange them in a division of undetermined rank and value, termed OSTRACODERMI or OSTRACOPHORI.

The simplest of these armoured types are *Pteraspis* and its allies, (Figs. 2, 3, 9), which have the head and abdominal region shielded both above and below, while the tail, in some cases at least, is covered with scales (3, 12). The tissue of the shield is peculiar, each plate consisting of an outer and inner hard layer with a relatively thick middle layer of polygonal chambers, through which the sensory canal-system of the skin forms ramifying delicate tunnels. There are no bone-cells in either layer of the shield, and the external surface is always concentrically striated. The eyes (Fig. 3, *orb.*) are widely separated, being placed laterally; and towards the hinder border of the dorsal shield there is an aperture on each side, evidently to allow the water to escape from the gill-cavity. The nostrils must have been placed close to the mouth on the under surface of the head; but of both mouth and nose nothing is known.

When the dorsal shield of one of these organisms—especially of *Cyathaspis* (1, 3, 12)—is examined from the inner (or visceral) face, it shows a number of symmetrically arranged impressions, which are also of interest from the possible clue they afford to the arrangement of the soft parts (Fig. 2). A pit in front (*pin.*) between the ordinary eyes probably lodged a rudiment of the pineal eye; and a paired series of lateral indentations (*br.*) is suggestive of the original presence of well-separated gill-pouches, between which it is reasonable to infer

EXPLANATION OF PLATE.

Fig. 1.—*Palæospondylus gunni;* skeleton, twice nat. size. Lower Devonian, Caithness. *au.* auditory region of skull. *l.* supposed lip-cartilage of mouth. *p.* processes. *s.* dorsal shield. *v.* ring-vertebra, end-view.

Fig. 2.—*Cyathaspis sturi;* inner view of dorsal shield. Upper Silurian, Galicia. *au.* supposed impressions of auditory organs. *br.* impressions of branchial sacs. *pin.* impression of pineal body. [*After* Alth.]

Fig. 3.—*Pteraspis rostrata;* upper view of dorsal shield. Lower Devonian, Hereford shire. *orb.* orbit. [*After* Lankester.]

Fig. 4.—*Trematapis schrenki;* dorsal and ventral (4*a*) aspects of shield. Upper Silurian, Isle of Oesel. *ant.* antorbital sense-organ. *orb.* orbital opening. *p.o.v.* postorbital vacuity. *x.* vacuity (?) [*After* F. Schmidt.]

Fig. 5.—*Cephalaspis lyelli;* upper view of dorsal shield. Lower Old Red Sandstone, Forfarshire. *n.* undetermined rows of pits (seen only on the inner face of the shield). *orb.* orbit. *p.o.v.* postorbital valley. [*After* Lankester.]

Fig. 6.—*Auchenaspis egertoni;* upper view of head and trunk. Lower Old Red Passage Beds, Ledbury. *orb.* orbit. *p.o.v.* postorbital valley.

Fig. 7.—*Pterichthys milleri;* dorsal, ventral (7*a*), and lateral (7*b*) aspects of head and trunk. Lower Old Red Sandstone, N. Scotland. *ap.* appendages. *j.* joint in appendage. *m.* supposed upper jaw. *op.* operculum. *orb.* orbit. [*Partly modified after* Traquair.]

Fig. 8.—*Acanthaspis decipiens;* ventral aspect of shield. Lower Devonian, Spitzbergen. *ap.* appendage. *pr.* process supporting appendage.

there were supporting elements of the nature of gill-arches. A pair of >shaped impressions (*au.*) between these markings are exactly such as might result from contact with ridges upon the auditory capsules, due to a great development of the upper semicircular canals, as in sharks. In short, it seems most probable that the shield in question covered a truly fish-like head; and if it were not for the invariable absence of limbs and jaws, and the forward position of the breathing apparatus in *Pteraspis* and its allies, these animals might be placed, without hesitation, in the class of fishes. The possibility that limbs and jaws were present, but not calcified enough to be preserved, must, however, be borne in mind; while the negative evidence on this subject, and the want of information as to the nature of the tail, are factors necessitating caution in determining affinities at present.

The next group of Ostracoderms (3, 6, 12) is typified by the genus *Cephalaspis* (Figs. 5, 10), and is remarkable for the manner in which its members mimic in outward shape the contemporaneous aquatic arachnids (*Eurypterus*, &c.) The anterior dorsal shield (Fig. 5) which was doubtless confined to the head and branchial region, consists of a single piece, rounded or tapering in front, abruptly trun-

FIG 9.—Restoration of *Pteraspis*. (From Brit. Mus. Catal. Foss. Fishes.)

cated behind, and with the rounded margin reflexed inwards below to form a flattened and ornamented rim. Like that of the Pteraspidians, the substance of the shield consists of three superposed layers; but the inner layer is bony, the thick middle zone is solid though traversed by a coarse network of blood-vessels, and the outer surface is covered with a dense tuberculated tooth-like tissue. The eyes (Fig. 5, *orb.*) are placed close together in the middle of the shield; and there is a large "postorbital valley" (*p.o.v.*) behind them, really a vacuity in the shield filled with a separate plate of peculiar structure (see No. 14, p. 4). There are no apertures for nostrils, which must thus have been situated on the lower side of the head close to the mouth, and of this nothing is as yet known. At the back of the shield, on each side, occurs a small flap stiffened with little plates (Fig. 10) evidently to be regarded as the operculum; and immediately behind the shield the ordinary scaling of the body begins, without any special development for the support of limbs. Paired fins are, in fact, absent; and the Cephalaspidians possess only a system of median fins represented by a dorsal and a caudal, not stiffened by fin-rays but by little elongated scales. The great deep quadrangular body-scales form a series of interlocking rings, no doubt corresponding to the series of muscle-plates of the trunk.

In some genera of Cephalaspidians, as *Auchenaspis* (Fig. 6) and *Didymaspis*, a few of the anterior body-scales fuse together into a continuous plate above and below, and that again is usually fixed to the head-shield; but even in such a case there is no appearance of a shoulder girdle. The advance in this direction is greatest in the genus *Trematapis* (Fig. 4), which exhibits no dividing line between the original head-shield and the coalesced body-scales; and the armour thus named is further of special interest, because the small bone between the eyes (*orb.*) is loose, not fixed, and because the postorbital valley (*p.o.v.*) seems to be without an infilling plate. In *Trematapis*, also, the small antorbital organ (*ant.*) common to all Cephalaspidians, is distinctly perforated, and the view has recently been expressed by Rohon (6) that this corresponds with the special sense-organ—"stirnorgan"—of Selenka (7).

From the highly-specialised *Trematapis*, we may pass to the third great division of the Ostracoderms, of which the familiar "winged-fish" (*Pterichthys*) of Agassiz and Hugh Miller may be taken as type. These organisms have been most carefully investi-

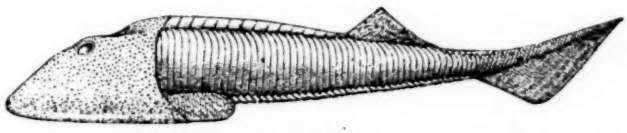


FIG. 10.—Restoration of *Cephalaspis murchisoni*, from specimens discovered by G. H. Piper, Esq. (From Brit. Mus. Catal. Foss. Fishes.)

gated by Dr. R. H. Traquair (8), and the restorations of *Pterichthys*, given in Figs. 7, *a*, *b*, are very slightly modified from his drawings. The shield, it will be observed, is essentially similar to that of those Cephalaspidians in which some body-segments are fused together; but the head is sharply separated from the trunk, and the armour, instead of being simple, is composed of a number of symmetrically arranged overlapping plates. It is also remarkable for the great pair of pectoral appendages (*ap.*), which are hollow, moveably articulated, consist of many plates, and are once jointed (*j.*). The small plate between the eyes (*orb.*) is loose, and marked inside with a pit that seems to have lodged the rudimentary pineal eye; a cheek-plate on each side is moveable (at least in *Pterichthys*), and probably formed the operculum (*op.*); while a pair of loose jaw-plates at the front of the head below (Fig. 7 *a*, *m.*) are finely toothed on their posterior border (at least in *Bothriolepis*). Of the precise nature of the nose, mouth, and jaws, however, nothing is as yet known. Behind the plated armour in *Pterichthys*, the tapering, scaly tail is often preserved, and the median fins are arranged exactly as in *Cephalaspis*.

The question now arises as to the true nature of the pectoral appendages, which form so distinctive a feature of *Pterichthys* and its allies, as compared with the two lower groups of Ostracoderms

already treated. They are commonly regarded as homologous with the paired pectoral fins of fishes, and certainly in their mode of fixation to the trunk there is much to favour that supposition; but the recent discovery (13) of an allied organism, *Acanthaspis*, in the Lower Devonian of Spitzbergen, suggests the wisdom of suspending judgment. The dorsal body-shield of *Acanthaspis* is simpler than that of *Pterichthys;* but the arrangement of the plates on the ventral aspect (Fig. 8) is identical, and there are also hollow pectoral appendages. These appendages, however, are merely simple triangular spines (*ap.*) firmly fixed by suture to processes of the body-armour (*pr.*); and if they are an inferior or special condition of the ordinarily moveable arms, it seems not unlikely that those arms will prove to be different in origin from the paired limbs of fishes.

In conclusion, it may be said that recent researches tend towards demonstrating the antiquity of the race of lampreys and hag-fishes; and evidence is accumulating to show that a great group of limbless, armoured Chordata formed a conspicuous feature in early Palæozoic life. Beyond this statement, however, we cannot yet advance, and much has still to be learned even concerning those few organisms whose main features have already been elucidated.

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A. SMITH WOODWARD.

IV.

Recent Observations on the Anatomy and Development of the Brachiopoda.

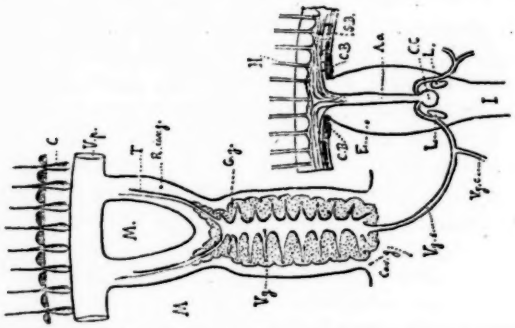
SOME important observations on the circulatory system of the Brachiopoda will be found in an interesting memoir recently presented to the Zoological Society of France (1), in which Dr. L. Joubin, Assistant Professor at the Rennes Faculty of Sciences, describes the anatomy of *Waldheimia venosa*, Solander, from the Straits of Magellan—the largest but anatomically least known species of this genus of the recent Brachiopoda. The specimens investigated were obtained from Mr. P. D. Œhlert, of Laval, and by exchange from the Zoological Department of the British Museum. It may, therefore, be assumed that the species was correctly identified, although neither figure nor description is given. *Waldheimia venosa* is a broad, globose, variable species, with a large circular foramen which mainly distinguishes it from *W. lenticularis*, Deshayes, from New Zealand.[1] To this robust form it is closely allied. In fact, Mr. W. H. Dall (2) considers typical examples of *W. lenticularis*, Deshayes, "as a local race representing for New Zealand the Patagonian *venosa* of Solander," which enjoys, of course, priority of nomenclature. The American authority, it may be added, would also include *W. venosa* (under the generic appellation of *Eudesia*) in the subfamily Terebratulinæ. *Waldheimia venosa* bears some external resemblance to another large brachiopod, the *Laqueus californicus* of Koch, from the North Pacific region, but the "snare-like" loop of this fine species when adult is quite different, and it belongs to the subfamily Terebratulinæ. It is certain that the size of the foramen of the articulated Brachiopoda is not a safe specific character, for it is much affected by the quiet or troubled condition of the waters and the nature of the sea-bottoms they inhabit.

Dr. Joubin had previously given considerable attention to the lyopomatous (3) and arthropomatous (4) brachiopoda while attached to the Arago biological laboratory at Banyuls-sur-Mer. An English

[1] A smaller species, *Wald. kerguelenensis*, Dav., was dredged by the "Challenger" in abundance off Marion Island and W. of Kerguelen Island. It is related to *W. lenticularis*, Desh., which also occurs in a fossil condition in the Upper Tertiaries of New Zealand.

abstract of the most important of his observations on the anatomy of *Crania turbinata*, Poli (=*C. rostrata*, Hoeninghaus) is appended to Davidson's posthumous Monograph of Recent Brachiopoda (5).

The study of the circulatory system of the Brachiopoda, Dr. Joubin remarks in the present memoir, is intimately connected with that of the digestive system. It is nothing more than an absorbent lymphatic system with a propellent heart, which directs the nutrimental fluid derived from the intestine towards the organs of the animal. He passes briefly in review the opinions expressed by Hancock and the older authors who maintained the existence of a regular circulatory system independent of the digestive apparatus, and those of more modern investigators, who absolutely denied the existence of any regular circulatory system. Dr. Joubin held this view so far as *Crania anomala* was concerned, and affirmed that he could not discover any structure in that genus comparable



Figure, partly diagrammatic, showing the dorsal surface of the stomach, the base of the arms, a fragment of the dorsal mantle, and the circulatory apparatus of *Waldheimia venosa*, Sol. (after Joubin). *A.O.*, aorta; *C.C.*, central heart; *S.B.*, brachial sinus; *H.*, cirri; *E.*, stomach; *I.*, intestine; *L.*, lateral hearts; *V.g.s.*, the superior dilated genital blood vessels; *V.g.i.*, inferior dilated genital blood vessels; *G.g.*, genital gland; *M.*, mantle; *Cav. g.*, general cavity; *T.*, terminations of the blood vessels; *V.p.*, peripheral lacune; *C.*, cilia; *C.B.*, brachial canal at the opening into the general cavity.

to the circulatory system as described by Hancock. But it is his intention to make further observations in this connection, as his researches on the better developed species *Waldheimia venosa* have enabled him to recognise the presence of a true vascular system in *Waldheimia*. He asserts that the heart is in direct relation with the small and numberless lacunes which surround the intestine. These channels are filled with lymph and white globular bodies charged with nutriment. They all terminate at the heart, which is a contractile organ, of which he finds Hancock's description to be absolutely correct in detail—the disposition of the muscular fibres alone excepted. The "arteries" described by that author differ both in essential structural characters and the nature of their contents from

true arteries. Dr. Joubin considers them to be lymphatic channels (lacunes) with a specialised function.

The digestive canal of *W. venosa* differs apparently but little from that of other articulated Brachiopoda. The general dimensions are described as larger, and the constituent parts as more clearly defined than in the smaller species. The mouth is formed by a shallowing of the digestive tube, ultimately terminating in an orifice within the brachial groove. The stomach is a large, deep pocket, with thick side walls. He notes the occurrence of a papillary prominence of the digestive epithelium, situated just beneath the vessel efferent from the heart, which may possibly serve as an organ of taste, but does not absolutely affirm that it is one. The so-called "liver" in this species is described as remarkably developed. It is the largest organ and attains relatively an enormous size. There are two hepatic canals in each lobe, the anterior is the larger. The cells present well-marked characters. Some are mucus cells, others granular. He considers that these two kinds of cells secrete distinct digestive ferments and that mucus is secreted by the calciform cells. As the hepatic fluid yields none of the elementary characteristics of bile, as understood among the superior organisms, and contains distinctive products not found at all in the biliary fluid, Dr. Joubin would change the name of this secreting organ into that of "stomacal gland," maintaining that the so-called "liver" of the Brachiopoda presents none of the features characteristic of that structure in other animals.

The organs described as "accessory hearts" by Hancock were not found to be present in any of the specimens of *Waldheimia venosa* Dr. Joubin examined. He considers them as "accidental vesicles" anomalous and inconstant. The occurrence, however, is noted and figures and descriptions are given of structures which he names *supplementary hearts*. These differ completely both in structure and relations from the "accessory hearts" of Hancock. They cannot be otherwise designated, if the name of "heart" be applied to the principal organ, for their structure is identical, and they perform the same function, that of receiving the lymph originating from the intestine. These supplementary hearts are symmetrically situated, the one on the right and the other on the left of the posterior region of the stomach. The organs are in reality oval-shaped blood-channels, larger and more dilated than in general. They are usually filled with white corpuscles. The principal heart receives and the median aorta conveys the lymphatic fluid of all the dorsal region of the stomach, the "supplementary hearts" collect that derived from the inferior and lateral regions, the nutrimental fluid passes through all these organs. The first, or principal heart, nourishes the brachial system; the two latter the genital organs *viâ* the two main lateral blood-vessels, which alone are truly vascular. These are composed of an external epithelium, a central layer of amorphous tissue, and an internal epithelium most distinctly characterised.

Dr. Joubin gives figures and descriptions establishing the course of the circulation in *Waldheimia venosa*. It becomes apparent that the lymph leaves the central heart and is received by the central aorta and the two lateral vessels, reinforced *en route* by the stores collected in the two supplementary hearts; it courses through all the vascular ramifications of the genital glands, falls into the general body-cavity, and bathes the exterior of all the organs. The function of the central heart is that of propelling the lymph in order that it may bathe the innumerable pores of the digestive system. "It is, therefore, strictly defined, a lymphatic heart intimately connected with the digestive apparatus from which it is a derivation."

Dr. Joubin offers in conclusion some observations of less importance relative to the special histological structure of the voluntary muscular system of *Waldheimia venosa*, which in all other respects resembles that of other species of the genus. The muscles are minutely striated. His memoir is well illustrated with twenty-six figures. The diagram of the course of the circulation in *Waldheimia venosa* (see p. 604) would be more readily understood if the lettering were not reversed. It is a welcome instalment of the author's researches on the general morphology of the recent Brachiopoda.

The philosophical palæo-biologist, or student of "still life," may be said to commence his researches at the point where the embryologist, or student "from the life," ceases his observations. It is only by a combination of the best results of both methods of investigation that we can hope to deduce a rational and enduring systematic classification of the Brachiopoda, and to solve the problem of their evolution. A remarkable uniformity has already been established in the embryology of the ancient Silurian types and their modern descendants in the memoirs of Morse, Brooks, Beecher, and Clarke. Adult conditions of ancient fossil species are shown to represent the youthful and adolescent stages of more modern forms. Interesting observations published at intervals during the last three years on the "Development of the Brachiopoda," by Mr. Charles E. Beecher, of Yale College Museum, and Mr. John M. Clarke, Assistant Palæontologist of the State of New York, mark a new departure in this direction (7). These are based on a careful study of the principles of growth, the acceleration of development, and the effects of mechanical genesis. They record the geological sequence of genera and species, and incidents in the ontogeny or development of the individual and the phylogeny of the race.

In a recent article (9), Mr. Beecher describes the stages of growth and decline, and applies to the Brachiopoda the nomenclature and methods first employed by Professor Alpheus Hyatt in his remarkable memoirs on the classificatory values of these stages for the Mollusca in general and a branch of the Cephalopoda in particular (Genesis of the Arietidæ). The early embryonic stages of the Brachiopoda are thus defined:—1, Protembryo; 2, Mesembryo; 3, Metembryo; 4,

Neoembryo; the true larval stage as Typembryo, with the addition of Dr. R. T. Jackson's term Phylembryo for the later phases of the last-named stage, when the animal, for the first time, can be referred definitely to the class to which it belongs.

It is at the close of the phylembryonic stage that the palæontologist *per se* should commence his investigations, for the initial shell, the protegulum, is then developed. There is a remarkable similarity in its appearance in all forms. It is noteworthy that "representatives of such distinct groups as *Rhynchonella*, *Spirifer*, *Athyris*, *Nucleospira*, and the Meristoids resemble each other so closely at this stage that the genus can be then determined only by comparatively trivial features" (7). Nearly twenty years ago, Morse pointed out the Linguloid form of the shell assumed in "the early stages of *Terebratulina*." It now appears that the adolescent *Shizocrania* resembles the youthful age of *Orbiculoidea*, and that the young *Orbiculoidea* recalls the genus *Paterina* in the "paterina stage" of its development. The protegulum has been observed by Mr. Beecher in forty genera of the leading members of the class, recent and fossil (8). It is described as always corneous and imperforate. The genera with pedicles sufficiently long to admit of free axial movement have elongated and rostrate shells (8). Breadth of form is the correlative of a short pedicle, while the discinoid resulted from mechanical conditions of growth.

The brachiopod next passes through its youthful or "nepionic" stage, when the shell is smooth, or with few plications, into the adolescent or "nealogic" period, and attains, at maturity, the "ephebolic" condition—the maximum of individual or ontogenetic development. Varieties and abnormalities usually begin to express themselves early in the development of the shell, and we learn that "such divergence from the normal form rapidly increases as maturity approaches (7), and they are gradually diverted from the harmony characterising the incipient stages of other members of the group." Soon after becoming adult, they commence to age, and pass through first a "clinologic" stage of arrested growth, and, subsequently, the "nostologic" phase of absolute senility.

These latter stages are well represented in the well-known, widely-distributed lobate Silurian orthoid *O. biloba*, Linné, by the disappearance of the bilobed condition in old age, as Mr. Beecher has fully described in his paper on the "Development of Bilobites" (10). In the degenerate form from the Lower Helderberg, designated by Conrad as *Orthis biloba* var. *varicus*, the features of extreme lobation, so characteristic of the adolescent and mature conditions of the *Orthis biloba*, are ultimately re-absorbed, and this variety differs at last but little in shape from the non-lobate orthids. The Gothland form known as *O. biloba* var. *verneuilianus*, Lm., presents a tendency to vary in the opposite direction. The lobation becomes more and more pronounced with increasing age, and the shell exceeds in size the normal species *Orthis biloba*, which is the directly linear or epacmic form, and enjoys

the widest distribution. The varieties *verneuilianus*, Lm., and *acutilobatus* (Ringueberg) are acmic or indirect divergents originating from the "nealogic" period of *O. biloba*. The variety *varicus* is the senile, a paracmic condition derived from the "ephebolic" or mature state of that species. In fact, the ontogeny of this very variable species, which begins life and ends it as a normal orthid, indicates its position as an aberrant species of that genus, and as its abnormality—the bilobed condition—was clearly defined in the specific name, we scarcely see the necessity for removing it from the genus *Orthis*, as defined by Dalman and established by Davidson, who declined to recognise the bilobate species and its varieties otherwise than as a genuine species of the genus *Orthis*. There seems even less necessity to disguise so well-known and much-studied a brachiopod as the living *Thecidium mediterraneum*, Risso, under the sub-generic designation of *Lacazella*. If biologists and palæontologists continue to indulge their Adamite denominative propensities in this direction, the student of the future will lose much time in re-establishing the identity of text-book genera and species.

Important conclusions are derived from comparisons of the geratologous phases of brachiopodal development when ornamental and other indications of maturity disappear in inverse order to their introduction. Age is expressed in the thickening of the shell, which takes place after the individual has reached adult size, and produces a varicose aspect, or truncated margins, and plications gradually vanish. There is an increase in the vertical diameter, the beak becomes involuted, and the deltidium, or deltidial plates, may be re-absorbed. The revolutions of the spirals become thicker and more numerous in the spire bearers. In the senile condition, or final nostologic phase of life, brachiopods, like human beings, shed their ornamental appendages and revert in extreme old age to the characteristics of their "nepionic" or youthful period. Such incidents (like premature baldness) may even be accelerated and transmitted. If we comprehend Mr. Beecher's argument correctly, the genera and species developed by the processes of "accelerated geratologous heredity," such as *Strophalosia* and *Austo-leges* among the Productidæ, generally occur in families after the maximum of genera and species is attained. He cites *Gwynia*, *Platydia*, *Cistella* and *Kraussina* among the recent Terebratuloids and *Atretia* among the Rhynchonellidæ as examples of such nostologic types (9). This view clears up many difficulties. The relative affinities of *Gwynia* and *Platydia* were left undetermined in Davidson's "Monograph of the Recent Brachiopoda" (12). By some authors *Gwynia* and *Atretia* are regarded as immature forms of other genera. But the swollen appearance of the margins of the shell is very apparent in the Australian species (*Atretia brazieri*, Dav.) (6), and indicates maturity. Mr. Friele deduced a similar conclusion as to its generic value from distributional data, as it was dredged in northern waters in great abundance where no specimens of *Rhynchonella* were ever obtained.

The wide distribution in the North and South Atlantic and the South Pacific Oceans of this geratologous type renders it additionally interesting. It is evident that it is not always an easy matter to distinguish between the youthful and senile brachiopod, for the Brachiopodist meets with the same difficulty as the philologist treating of the Chinese language, namely, that of determining whether it is in its first or second childhood.

While fully admitting certain resemblances between the typembryo of the brachiopod and the larval stages of *Spirorbis*, Mr. Beecher refers to the existence of several important differences. He shares the opinion expressed by Joubin, Œhlert, Balfour, and other recent investigators, that the Brachiopoda constitute a distinct and independent class. There seems to be a general reaction in favour of this view, consistently upheld throughout by Dall and Davidson. King was the first to express the opinion that the articulated Clistenterata were the degenerated descendants of their aniferous predecessors, the Tretenterata. But it has always been difficult to account for the presence of an anal opening in the hingeless forms, and its absence in the articulated genera. In this connection the facts are justly emphasised by Mr. Beecher, that the dorsal beaks of *Amphigenia*, *Athyris*, *Atrypa*, and *Rhynchonella* are usually notched, or perforate from the union of the crural plates above the floor of the beak, leaving a passage through to the apex, and that a similar opening occurs between the cardinal processes in *Strophomena*, *Stropheodonta*, and allied genera with a furrowed deltidium, as in *Leptæna*. "This character, which cannot be considered as in any way connected with the pedicle opening (9), points to the existence in the early articulate genera of an anal opening *dorsal to the median line*," a character which Dr. Joubin first demonstrated is peculiar to *Crania* (3, 5) among the recent species. This, the "dorsal foramen" of King and Hall, is also known as a "visceral foramen." Mr. Œhlert has suggested that it was probably occupied by the terminal portion of the intestine.

Mr. Beecher considers the group hitherto known as *Arthropomata* (Owen) or *Clistenterata* (King) as retrogressive so far as the loss of eyes and the closure of the intestine are concerned; progressive mainly through expansion of anterior elements and limitation of pedicle opening to one valve.

Davidson (11), in his masterly "Summary to the British Fossil Brachiopoda," published in 1884, classified the Brachiopoda in general into fourteen named families and two unnamed ones, containing, respectively, obscure genera with uncertain affinities. In 1891, Beecher legitimately proposed a new classification, based in a great measure on the nature of the pedicle opening, its limitations and associated modifications (8). He divided the class into four orders. The first and second correspond with the *Lyopomata* of Owen = *Tretenterata*, King. The third and fourth with the *Arthropomata* of Owen = *Clistenterata*, King.

1.—The ATREMATA, in which the pedicle is free, embraces the genera included in Davidson's family groups of the Lingulidæ and Obolidæ and the Silurian Trimerellidæ.

2.—The NEOTREMATA, which comprises the majority of genera in the Discinidæ, Craniidæ, and Siphonotretidæ as established by Davidson.

3.—The PROTREMATA includes the Orthidæ, Strophomenidæ, Porambonitidæ, Productidæ, and Thecidiidæ.

4.—The TELEOTREMATA contains the Rhynchonellidæ, Spiriferidæ, and Terebratulidæ of that great authority.

This scheme possesses the merits of natural grouping from a phylogenetic standpoint. Waagen and Beecher concur in regarding *Orthis* as the lowest and simplest form of the articulated genera. Mr. Beecher believes *Kutorgina* to be related to *Orthisina* and *Strophomena*, of which it is the ancestral type. This is the main point of difference between Davidson and Beecher, so far as the relations of genera and families are concerned, as the former never included *Thecidium* with the Terebratulidæ. Mr. Beecher considers this genus as descended from ancestors with a habit of fixation, so far back as the Silurian, and that its affinities are with the Strophomenoids, which have also a single plate deltidium. *Thecidium* bears, in his opinion, "the same relation to other Brachiopoda that Ostrea bears to Avicula among the Pelycypoda." It may be added that M. Eugène E. Deslongchamps (15) regarded the division between the Rhynchonellidæ and Spiriferidæ as much less absolute than at first apparent. He cites the recent spinose *Rhynchonella* from Japanese waters (*R. Döderleini*, Dav. MS.) as bearing less resemblance to the Acanthothyrid, or spiny Rhynchonellæ of the Jurassic seas, than to the genera *Spirigera* and *Atrypa* of the Palæozoic oceans. The only known examples of this remarkable "survival" are preserved in the Strassburg Museum, and "a good haul" of this species is much to be desired in the interests of science (13).

Davidson and King showed very clearly the relations of the Trimerellidæ with the Lingulidæ, while admitting that from the rudimentary characters of the hinge and other external features, they might be regarded as related to the Clistenterata (14). They appear, in fact, to be somewhat intermediate in structure between the two orders. Beecher and Clarke consider it not improbable that phylogenetic development tended in two main channels, one leading through *Strophomena*, *Orthisina*, *Leptæna*, *Chonetes*, *Productus*, &c., and the other in the direction of *Rhynchonella*, *Spirifer*, *Atrypa*, *Retzia*, and *Terebratula* (7).

The promised continuation of Mr. Beecher's researches on this subject will be awaited with interest. The results already published should induce "species-mongers" to pause in the manufacture of specific designations for ill-defined species, often representing different

ages of growth, or local variations, resulting from changed environment. But we are not sanguine.

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AGNES CRANE.

V.

Botanical Nomenclature.

FOR some considerable period, questions connected with botanical nomenclature have been agitated with an energy which may have appeared to some entirely disproportionate to the importance of the subject. The Laws of Botanical Nomenclature, drawn up by M. Alphonse De Candolle, and adopted by the International Botanical Congress held in Paris in 1867, were, as might have been expected, subjected to criticisms from various quarters. These criticisms have been carefully considered by M. De Candolle, and the more important of them were dealt with in his pamphlet, "Nouvelles remarques sur la Nomenclature Botanique," published in 1883, and embodied in a new edition of the Laws appended thereto.

The law of priority, as it has been called, is accepted as a general principle by all botanists, and, of course, by all zoologists also; my remarks throughout this paper are entirely confined to the botanical side of the question. But when the law comes to be applied, differences of opinion are at once manifest. Is a generic name which has become familiar by frequent or even by general use, to be superseded by an earlier and disused synonym? And, in the case of a species, is the oldest published specific name, no matter in what genus, to be thereafter inseparable from the plant? or is the name by which the plant was first placed under the genus in which it is now retained to be regarded as its proper appellation? It would seem, at a cursory glance, that both of these questions might easily be settled, and that there should be no difficulty in arriving at a conclusion which would commend itself on the score of logic, common-sense, and convenience. But this conclusion is, unfortunately, not justified by events.

It would be impossible in a paper like this to deal with the numerous points which have arisen in connection with botanical nomenclature. A treatise of considerable bulk would be needed to do justice to the subject. I will therefore only, as briefly as possible, deal with the questions raised above, pointing out in doing so the various considerations which have brought about the existing state of chaos, and giving, in conclusion, the lines which have been laid down to conduct us back to a sane mode of procedure.

I.—Generic Names.

During the last century and the early part of the present, when workers were few and isolated and means of publication and communication restricted, it was inevitable that a considerable crop of synonyms should arise. Later, certain botanists seem to have assumed an arbitrary power, by virtue of which they superseded names of less popular men, on grounds which could only have been tolerated from their eminence of position, or from a failure to recognise the rights of priority. Thus Robert Brown substituted *Xerotes* (1810) for Labillardière's *Lomandra* (1804) without assigning any reason for the change; and superseded R. A. Salisbury's *Chlamysporum* (1809) by his own *Thysanotus* (1810) for reasons which would not satisfy modern requirements. Salisbury, indeed, as I have shown at some length elsewhere,[1] was the victim of something very like a conspiracy to ignore, as far as possible, his work and his names; and it is certain that many of the latter must be restored, and take precedence of Smith's and Brown's. Whether this will be the case with his Proteaceous genera, published under peculiar, and I fear not over-creditable, circumstances,[2] may possibly be open to question; but that *Hookera* of Salisbury (1808) must replace *Brodiæa* of Smith (1820) is absolutely certain. If the well-known moss-genus *Hookeria* be considered as approximating too closely in name to *Hookera*, that also must disappear; for Salisbury published *Hookera* in March, 1808, while the paper in which *Hookeria* was established was not even read until a month later.

Smith's division of the Linnean *Nymphæa* (in 1808–9) into two genera, for one of which he retained the Linnean name, proposing *Nuphar* for the other, was anticipated by Salisbury in 1805–6, who called the white-flowered species *Castalia*, leaving *Nymphæa* for the yellow-flowered plants. But Smith, supported by Goodenough, deliberately set aside Salisbury's names "upon irrefragable [*i.e.*, classical] grounds," [3] and, with very few exceptions—*e.g.*, Rees's Cyclopædia—his names were accepted by all subsequent botanists until Professor E. L. Greene called attention to the necessary revival of the earlier titles. The tardy restitution of Salisbury's names has caused some trouble and confusion, but for this Smith and his followers must be held responsible. Salisbury was undoubtedly, and perhaps deservedly, unpopular; but this cannot justify the wholesale boycotting of his names.

A similar course of action has been adopted with regard to Rafinesque, whose eccentricities have been allowed to obscure his undoubted genius. Our American friends, to whose methods of procedure I shall have to refer later, have devoted much energy and research to the unearthing and rehabilitation of Rafinesque's names,

[1] *Journal of Botany*, 1886, pp. 49, 296. [2] *Loc. cit.*, p. 297.
[3] *Loc. cit.*, 1888, p. 7.

and, although I cannot follow them entirely, it is certain that many of his names must be restored. The genus *Echinocystis*, founded by Torrey and Gray in 1840, upon *Momordica echinata*, Muhl., was anticipated by *Micrampelis*, published by Rafinesque in 1808, with a diagnosis and an indication that it was based on the same species; and many other examples might easily be cited.

It cannot be too much regretted that the great opportunity for establishing an enumeration of genera which would have complied with the requirements of the law of priority should have been neglected. The publication of Bentham and Hooker's *Genera Plantarum* afforded this opportunity, but the learned authors, although in many instances aware that there was an earlier name than that which they adopted, deliberately placed this as a synonym. Had the contrary course been adopted, it is hardly too much to say that, so far as genera are concerned, our nomenclature would, by this time, have been established upon a solid basis. Instead of this, however, the illustrious sanction thus given to the retention of names admittedly not the oldest, and the adoption of a similar limitation and naming of genera by so many subsequent authors, has placed these names in a position of false authority. It is true that, more and more, these false names are being set aside by individuals, but in the *Genera Plantarum* the whole work might have been done once for all. The various portions of Colonial Floras which issue from Kew have, for the most part, followed the *Genera* in nomenclature; and, worst of all, Mr. B. D. Jackson's great *Index*, which is making satisfactory progress, follows the same lines, although the dating of each name makes it plain which should be retained. It is scarcely an exaggeration to say that many thousands of names which stand in the *Index* will have no existence, save as synonyms, so soon as any monographer takes the trouble to restore the right names of the genera; and for this unnecessary distention of our already overcrowded list the authors of the *Genera* must be held responsible.

It is right to say that this ignoring of the oldest names was justified by Mr. Bentham, and has lately found a defender in Mr. W. B. Hemsley, of the Kew Herbarium. According to the former, "names which have been long and universally adopted must be considered as having acquired a right of prescription to overrule the strict laws of priority. It would, indeed, be mere pedantry, highly inconvenient to botanists, and so far detrimental to science, now to substitute . . . *Fibichia* for *Cynodon* . . . or *Singlingia* [*Sieglingia*] for *Triodia*."[4] It is satisfactory to know that, in spite of this *ex cathedrâ* pronouncement, and of the weight deservedly attaching to Mr. Bentham's utterances, these two names, as I have pointed out elsewhere,[5] have been adopted in the last two issues of the *London Catalogue*, and *Sieglingia* appears in two English local floras.

[4] "Notes on Gramineæ," *Journ. Linn. Soc.*, vol. xix., p. 19, 1882.
[5] *Journal of Botany*, 1892, p. 54.

Mr. Hemsley[6] defends the action of Bentham and Hooker on the ground that as "the Kew botanists, for reasons of their own, took no part in the proceedings of the Congress" at Paris, they were "not bound down by the law of priority," and "were not always consistent, even from the point of view of expediency and convenience." He speaks as though the law of priority was first formulated at the Paris conference, and was, therefore, only binding on those who took part in it! I have reviewed Mr. Hemsley's defence elsewhere,[7] and need not dwell upon it at length; it is an elaborate plea for "convenience" as a principle, and, to my mind, carries with it its own condemnation. "Convenience" and "expediency" are, at best, but temporary, and no satisfactory or lasting structure can be raised on so shifty a foundation.

So far as we can understand, the plea is largely based on the inconvenience which the restoration of the right names would inflict upon—gardeners! The Kew botanists, Mr. Hemsley tells us, are "almost exclusively responsible" for "the botanical nomenclature current in gardens," and this must not be disturbed by scientific considerations. Yet Bentham and Hooker did not hesitate to relegate the plants known in gardens as *Gloxinia* to the genus *Sinningia*; and if this can be done in one case, it is not clear why it is impossible or objectionable in others. If Mr. Hemsley distinguishes between changes made on scientific and those made on literary grounds, he will hardly deny that the (temporary) resulting inconvenience is equal in both cases.

An example of what has taken place will make my meaning clear. In the *Genera Plantarum* the genus *Æschynanthus* of Jack is maintained, the earlier name *Trichosporum* of D. Don being placed as a synonym. Mr. C. B. Clarke[8] connives at this suppression, and even justifies it, in the following words: "*Æschynanthus*, Jack, was published in January, 1823; but *Trichosporum*, D. Don, being published July, 1822, has the right of priority; *Æschynanthus*, however, having been accepted for half a century, it *would not be expedient* to relinquish it." He proceeds to describe twenty-two species, eight of which are published for the first time. Later, Mr. Clarke monographed the whole genus,[9] still retaining the later name, and added thirty new species. Had the earlier name been taken up in the *Genera*, more than half the genus would have been described as species of *Trichosporum*; as it is, an already over-burdened nomenclature may be encumbered with a set of useless synonyms, so soon as a Dr. Taubert (*see* p. 619) shall come forward and substitute *Trichosporum* for *Æschynanthus* throughout the genus. And this is justified on the ground of "convenience" and "expediency"!

A brilliant opportunity for wholesale changes is afforded by the

[6] *Nature*, Dec. 24, 1891, p. 169.
[7] *Journal of Botany*, 1892, p. 53.
[8] *Fl. Brit. Ind.*, vol. iv., p. 337.
[9] *DC Mon. Phan.*, vol. v.

enunciation of the formula "once a synonym always a synonym."[10] This was promulgated by Dr. N. L. Britton in April, 1891,[11] and was hailed with enthusiasm by his fellow-reformer, Professor E. L. Greene, although it wrings from him the melancholy confession that after the many years during which they have laboured at correcting the errors of their predecessors, not even the beginning of a settlement has been attained. "Change and confusion," says Professor Greene, "are sure to be the order of the day so long as revertible names are not everywhere ignored." A name once employed and dropped, must never, under any circumstances, be used again. "The sooner systematic botanists agree upon the principle 'once a synonym always a synonym,' the earlier will botanical nomenclature *begin to be found* a settled thing." Professor Greene then proceeds, with his usual promptitude, to put his principles into practice, and at once gets rid of several well-known names, *e.g.*, *Torreya*, Arnott (1830), for which *Tumion*, Rafinesque (1840), is to be substituted, *Torreya* having been previously employed and rejected for other genera. This also gives him an opportunity for inventing entirely new names; *Osmaronia* is to supplant the well-known *Nuttallia* of Torrey and Gray, *Chrysamphora* supersedes *Darlingtonia*, DC., and so on. This new device for increasing an already redundant nomenclature is to apply to species as well as to genera, and if Dr. Britton, Professor Greene, and their friends are let alone, they will probably succeed in renaming the American flora three or four times over before botanical nomenclature becomes "a settled thing."

The priority of a name, according to Dr. Britton, is established in some cases by its position on a page! Thus, Adanson proposed the names *Tissa* and *Buda* for the group of plants since called *Spergularia* and *Lepigonum*. *Cæteris paribus*, it would have appeared natural, should either be revived, to adopt that which stood first on the page; but Dumortier, who first (in 1827) called attention to Adanson's names, preferred to adopt *Buda*, the second in position. Dr. Britton, however, considered that *Tissa* had a prior claim, inasmuch as it preceded *Buda* on the page, and he did not scruple to substitute *Tissa* where *Buda* had been employed.[12] His discovery of a yet earlier name, *Corion*, has not only set this question at rest, but has given Mr. N. E. Brown an opportunity of inventing another set of synonyms, to each of which his own name is attached.[13] "I accepted *Tissa* rather than *Buda*," says Dr. Britton, "for the simple reason that it stands first on the page in Adanson's *Familles*. That is priority, I am sure." Priority of *place*, yes; but will anyone seriously contend that one part of a page is *published* before another? If so, there is a fine

[10] *Garden and Forest*, vol. iv., p. 202. [11] *Pittonia*, vol. ii., p. 187.

[12] See *Journal of Botany*, 1890, pp. 157, 295.

[13] *Supplement to English Botany*, ed. iii., p. 48.

field open for much more extensive changes—*e.g.*, the botanists who, with Bentham and Hooker, unite *Amygdalus* and *Prunus* must adopt the former name for the aggregate, inasmuch as *Amygdalus* precedes *Prunus* in Linné's Genera!

II.—Specific Names.

Notwithstanding the examples just given, however, it is comparatively easy to arrive at a common basis of action with regard to the names of genera: those of species present far greater difficulties. To attempt anything like a complete summary of the points which have been raised would occupy considerable space, and although it would incidentally pay a gratifying tribute to human ingenuity, its practical utility would be slight. M. Alphonse De Candolle has dealt with the matter at length,[14] and I propose here only to refer to some of the views which have been promulgated since the publication of his essay. These have mostly proceeded from the new school of American botanists—men whose ability cannot be called in question, although their zeal in matters connected with nomenclature seems to me to outrun their discretion. We are all agreed as to the desirability of establishing a permanent specific appellation, and in condemning the practice, which at one time was not infrequent,[15] of superseding a name by one considered more appropriate. But how the permanent name is to be secured is a matter on which there is much difference of opinion; and the reformers—as is not uncommon with reformers—are by no means at unity among themselves. Another debated matter on which there is considerable difference of opinion concerns the authority to be cited for the name of a plant.

The main point at issue is not a new one, although it has gained new life from its recent presentment. Is the specific name first bestowed upon a plant to be maintained (with certain necessary exceptions) ever after, no matter to what genus the plant may be transferred? If so, who is to stand as the authority for the new combination? The latter point was definitely settled by the Decandollean Laws,[16] but in 1888 a sub-committee of the younger American botanists, headed by Dr. N. L. Britton,[17] formulated the desirability

[14] *Nouvelles Remarques sur la Nomenclature Botanique*, 1883.

[15] R. A. Salisbury and S. F. Gray are familiar examples. Nuttall is another instance. The plant usually known as *Impatiens fulva*, Nutt., had two earlier names—*I. biflora*, Walter (1788), and *I. maculata*, Muhlenberg (1818). Nuttall says: "As several species are spotted I have not adopted the last name, and changed the former because it was deceptive." (Gen. N. Amer. Plants, vol. i., p. 146).

[16] "For the indication of the name or names of any group to be accurate and complete, it is necessary to quote the author who first published the name, or combination of names, in question."—*Art.* 48.

[17] *A Preliminary Catalogue of Anthophyta*, etc. See *Journ. Bot.*, 1888, p. 257.

of following the practice of zoologists in maintaining the earliest specific name,[18] and proposed to cite in parenthesis the name of its original inventor, followed by that of the author who placed it in its present position. Thus, to take an example, the plant first placed by Pursh in its present genus *Pycnanthemum*, and generally known as *P. linifolium*, Pursh, was called by Linné *Thymus virginicum*. The sub-committee restored Linné's name and substituted for Pursh's name, *P. virginicum* (L.) B.S.P.[19] This, they were convinced, would "in the not distant future be generally accepted as the only law that promises a reasonable fixity of botanical names"; and Dr. Britton further commends it as "just, rational, and stable."[20]

The best comment upon the "stability" of this new and cumbrous mode of citation is that supplied by its chief promoter. In 1891 Dr. Britton threw in his lot entirely with the zoologists, and "would quote [only] the original author of the specific name, leaving the author of the binomial to be brought out in the synonymy of the species. "This," he adds, "has the advantage of doing away with the double citation"[21]—the "just, rational, and *stable*" method had endured for less than three years!

Not only is the specific name in ordinary cases to be held sacred, but it must remain even when it has been subsequently adopted for a genus separated from the type. Thus *Cochlearia coronopus*, Linn., was taken as the type of a new genus, *Coronopus;* and Gaertner, who established it, called the species *Coronopus ruellii*. This plant, according to Dr. Britton,[22] must be called *Coronopus coronopus* (L.); in 1888 he would have styled it "*Coronopus coronopus* (L.), Britton;" botanists generally would cite it (not without a feeling of irritation at the needless multiplication of synonyms) as "*Coronopus coronopus*, Britton." Here we find a very pretty difference of opinion among the leaders of the new school; for Professor E. L. Greene, who shares with Dr. Britton the glory of initiating the neo-American movement, sees in this duplication of names "a natural unfitness which all our sense of what is proper in nomenclature shrinks from."[23] Whereupon Mr. Conway Macmillan takes a hand in the game, and thinks Dr. Britton's suggestion "so excellent that it will scarcely fail of universal adoption after a season of recalcitrant objection";[24] and Mr. T. D. A. Cockerell, in adding another list of such combinations (in which, by way of contributing a little novelty on his own account, he adopts a small initial for the second name), goes one better by

[18] I am aware that the Laws prescribe the retention of the specific name when a plant is transferred to another genus. But I do not understand this to be retrospective. There has been considerable divergency of practice on this point, even among those accepting the Laws.

[19] *Journ. Bot.*, 1888, p. 295.

[20] *Loc. cit.*

[21] *Garden and Forest*, vol. iv., p. 202.

[22] *Bull. Torrey Bot. Club*, 1891, p. 268.

[23] *Pittonia*, vol. ii., p. 214.

[24] *Bull. Torrey Club*, 1892, p. 15.

pronouncing that "if the identical name for both genus and species is to be avoided, it is the genus that must go."[25] We may be thankful that this gentleman did not proceed forthwith to formulate a new series of generic names to meet the possibility thus indicated, as Professor Greene has done in his paper already referred to[26] (see p. 616).

The position now claimed for the specific name has been fully discussed in an admirable note by Professor L. H. Bailey—one of the ablest of the American botanists—published in the *Botanical Gazette* for 1891, p. 215. He applies the term "monomialism" to the new scheme, and says: "They are telling us that two words do not constitute the name of the plant, but that *the name, per se*, is the second word of the two. In other words, *Saccharinum* is the name of the sugar maple, and *Repens* is the name of the white clover. This is the monomial system of nomenclature, and its devotees are delving through every author in the hope of finding *the name* of the plant. When this name is found—or supposed to be found, which amounts to the same thing—it is attached to some generic name to which it was never designed to fit, and the twain, to which an algebraic formula has been attached, is given to the world as the monomio-binomial name of the plant." I cannot quote at greater length Professor Bailey's vigorous and well-timed protest, but his concluding sentence must be cited. "One of the most mischievous features of the whole thing is the ease with which authors of local floras obtain a cheap notoriety by making new combinations—which will likely be changed by the next cataloguer—and the extent to which it fosters the notion that making a new name and differing from an authority are the chief ends of systematic botany."

With regard to the citation of authorities, it seems to me that the name of that author is to be adopted who has first placed a plant in the genus to which it is recognised as belonging. This view is in accordance with the practice prevailing at Kew. There is, however, a manifest want of right feeling in the application of the rule in cases where a man has done nothing but transfer a plant from one genus to another, adding his own name to the new combination. In the *Botanisches Centralblatt* for 1891 (xlvii., pp. 385–395), Dr. Taubert restores a number of Aublet's names, which take precedence of those by which the genera have usually been known—among them *Swartzia*, *Derris*, and *Macrolobium*, which give place to *Tounatea*, *Deguelia*, and *Vouapa* respectively. He proceeds to enumerate the species, to which, in almost every case, he attaches his own name. Unless some justification can be found for disregarding them, it would seem that these names must stand; but Dr. Taubert's action is contrary to general practice, as well as to good taste. In cases where the restitution of an old generic name has been established, it is customary to leave the

[25] *Id.*, p. 95.

[26] *Pittonia*, vol. ii., pp. 185–195.

specific combinations as a whole to be dealt with by a monographer of the genus; and this has the further advantage of preventing an undue increase of synonyms, as the revision of a genus usually results in the sinking of a certain number of species, which thus do not require renaming. There is no evidence that Dr. Taubert has studied any one of the genera for the species of which he claims to stand as authority. The notion at one time prevailing in some quarters, that a certain amount of "credit" attached to the citation of the name of an author, is entirely set aside by such action as this.

One word must be said as to the citation of early (and purely imaginary) authorities for names, adopted by Professor Greene, whose enthusiasm for antiquity has led him into regions far more remote than his fellow labourer has ventured to tread. "*Lupinus*, Catullus"; "*Euphorbia*, Pliny"; *Amygdalus*, Theophrastus"; "*Linum*, Vergil"; are only samples of his charming restorations.[27] The accidental binomials employed before the binomial system was invented are also to be accredited to their original authors, so we must write "*Ranunculus flammula*, Dodoens"; "*Bromus sterilis*, Gerarde" (why does so scrupulous a stickler for accuracy give our Elizabethan botanist a final "e"?); "*Artemisia vulgaris*, Cæsalpinus," and the like.[28] Dr. Britton, however, with whom I am glad to find myself for once in accord, refuses to countenance Professor Greene's action, which appears to him "to be straining a point for history's sake."[29]

III.—A Proposal.

The rapid progress which botanical nomenclature was thus making towards a state of chaos hardly less formidable than that from which we were freed by Linnæus in 1737–53, was brought to a climax last year by the publication of Dr. Otto Kuntze's ponderous *Revisio Generum Plantarum Vascularium*. The individual pranks of American botanists might have been left to neutralise each other; but a work of 1,011 pages, preceded by an introduction of 155, and containing a specific renaming of more than 30,000 plants, called for prompt and decisive measures. Dr. Kuntze's book (which space will not allow me to notice fully) has been carefully analysed by Mr. B. D. Jackson in the *Journal of Botany*,[30] and has been reviewed at length in most foreign botanical periodicals. Its publication, indeed, has called forth a strong protest from the whole botanical world, with the exception of those Americans who, in some degree, share the author's views; and the veteran Alphonse De Candolle has taken a prominent part in the protest.

To the Berlin botanists, however, headed by Professors Engler,

[27] *Flora Franciscana* passim.
[28] *Pittonia*, vol. i., pp. 179–182.
[29] *Bull. Torrey Bot. Club*, 1891, p. 159.
[30] 1892, pp. 57–62.

Ascherson, and Urban, is due the credit of initiating a practical measure which, if adopted, will put a stop to future divagations, and, it may be, neutralise the mischief already wrought. They have sent out to the botanists of the world a memorandum which has already received the adhesion of a very large number, and which is likely, if adopted, to be productive of good results. The document is as follows:—

Since the time of Linnæus, botanists have continually endeavoured to gain a uniform nomenclature, and these endeavours were completely justified on account of an easier mutual understanding. We know very well that certain differences will always remain, because the decision on some questions only depends on the author's subjective opinion. But we hope that a gradual and continual reformation will bring an essential improvement. O. Kuntze's *Revisio Generum* has raised an evident perturbation, and will cause a complete confusion; therefore we thought it necessary to propose the following four resolutions, which refer only to the genera:—

I. The starting-point of the priority of the genera as well as the species is the year 1752, resp. 1753.

II. Nomina nuda and seminuda are to be rejected. Pictures alone, without diagnoses, do not claim any priority of a genus.

III. Similar names are to be conserved, if they differ by ever so little in the last syllable; if they only differ in the mode of spelling, the newer one must fall.

IV. The names of the following larger or universally known genera are to be conserved, though after the strictest rules of priority they must be rejected; in many of them the change of the names now used is by no means sufficiently proved. [This list I do not think it necessary to reprint here; it is likely to receive many additions.]

Ad. I. After Alph. De Candolle had proposed to take the year 1737 as the starting-point of the priority of genera many botanists had acknowledged it. But we think that the turning-point from the ancient botany to our modern science rests in the introduction of the binomial nomenclature. Therefore we propose, after a previous communication with Alph. De Candolle, to remove the starting-point for both, the species as well as the genera, as far as to the year 1753, resp. 1752, date of the *Species Plantarum*, ed. 1 (1753), with the 4th ed. of the *Genera Plantarum* (1752). Before that time the scientific position of Linnæus is not superior to Tournefort, Rivinus, and many other botanists, who often had described and segregated the genera more exactly than he did.

Ad. II. Many genera have been founded on a picture only, without a diagnosis. No doubt, by means of it a species sometimes can clearly be made out and recognised, and if the picture is a good one, all the characteristics of the plant can be observed. But a picture can never show the special characteristics alone, which raise the genus above the other of its affinity. A genus only gains priority by a verbal diagnosis, and *nomina nuda* and *seminuda* are to be rejected;

therefore the following works cannot claim a right of priority:—Rumphius, *Herbarium Amboinense* (1741–1755); Burmann, *Flora Indica* (1768); Patr. Browne, *History of Jamaica* (1756); Lamarck, *Illustration des Genres* pro parte, &c.

Ad III. There are to be conserved *Adenia* as well as *Adenium*, *Acnista* as well as *Acnistus*, *Alectra* as well as *Alectryon*, *Apios* as well as *Apium*, *Rubia* as well as *Rubus*, *Bellis* as well as *Bellium*, *Chloris* as well as *Chlorea* and *Chlora*, *Glyphæa* as well as *Glyphis* and *Glyphia*, *Calopogon* as well as *Calopogonium*, *Atropa* as well as *Atropis*, *Galax* as well as *Galaxia* and *Galactia*, *Danaë* as well as *Danais*, *Drimia* as well as *Drimys*, *Glechoma* as well as *Glechon*, *Hydrothrix* as well as *Hydrotriche*, *Micranthus* as well as *Micrantheum*, *Microtea* as well as *Microtus*, *Platystemma* as well as *Platystemon*, *Silvæa* as well as *Silvia*, &c.; we doubt that there is any scholar who will confound them. On the contrary, *Tetraclis* and *Tetracleis*, *Oxythece* and *Oxytheca*, *Epidendrum* and *Epidendron*, *Oxycoccus* and *Oxycoccos*, *Asterocarpus* and *Astrocarpus*, *Peltostema* and *Peltistema*, are only different modes of spelling the same word, and the newer one is to be refused, if they name different genera.

Ad IV. The impulse that led to the acknowledgment of the right of priority was only the vivid desire to create a stable nomenclature. If we see that by the absolute and unlimited observance of the principle we probably gain the contrary of what we intended, we, who have ourselves made the rules of priority as a law, have the right to amend the latter. Therefore we present a list of genera that have more than a merely scientific interest, or that are very large, and we propose to conserve them in spite of the rules of priority, in order to avoid a general confusion by the change of many thousand names.

When the memorandum was first submitted to us at the British Museum, we thought that the date 1737 might continue to be accepted for genera, and we wrote in that sense to Professor Engler. In his absence, Professor Ascherson replied, pointing out that this date had only been fixed by M. De Candolle in 1883, and that that botanist had no intention of implying that all genera established between 1737 and 1753, and ignored by Linnæus in the *Species Plantarum*, should be resuscitated. M. De Candolle now fully concurs in the proposals of the Berlin memorandum, and adopts the fourth edition of the *Genera* (1752) for genera, and the first edition of the *Species* (1753) for the species. The fact that the fourth edition of the *Genera* is not actually by Linnæus himself, is met by the statement that it was recognised by him as authentic, seeing that he styled the next edition, by himself, the fifth. After this explanation, the British Museum botanists withdrew their objection, and gave their corporate assent to the only article of the Berlin memorandum as to which they had had any doubt in the following terms:

"We do not see how we can stop short of 1737 when dealing with genera *as genera*. But as binomial names—and every specific name includes, of course, both the generic and the trivial designations—did not exist until the publication of the *Species Plantarum* in 1753, we agree that a name given in that work cannot be set aside for one the generic part of which was ignored or set aside by Linnæus; and we

therefore acknowledge that this practically fixes the date of genera to 1752."

Whether this solution of the difficulties which exist will receive the adhesion of a sufficient majority of botanists is at present doubtful: that some solution is needed is certain. The subject is well worth the attention of all who have at heart the establishment of a recognised standard of nomenclature on practical lines, and the present seems a favourable opportunity for settlement.

JAMES BRITTEN.

SOME NEW BOOKS.

FUR-BEARING ANIMALS, IN NATURE AND IN COMMERCE. By H. Poland. 8vo. Pp. lvi. and 392. With 16 illustrations. London: Gurney & Jackson, 1892.

WITH the exception of the "Fur-bearing Animals" of Dr. Elliott Coues, which deals only with the Mustelines of North America, and was published as far back as 1872, and Mr. H. W. Elliott's various works on the seal-fisheries of Alaska, the latest of which dates from 1882, we have scarcely any books devoted to wild animals considered chiefly from a commercial point of view. Consequently, the scientific naturalist is not unfrequently at a loss to know where to turn in order to obtain reliable information as to the number of any particular species of mammal yearly slaughtered for the sake of its pelt. We are, therefore, glad to welcome Mr. Poland's work, which appears to give all available information relating to the fur trade. The author gives a very long list of mammals of which the hides or pelts are more or less used in commerce; but as varieties or local races are placed under distinct headings, the total number of animals appears greater than it would be if they were arranged strictly under specific headings.

In the Introduction Mr. Poland gives us a valuable series of tables of the numbers of furs imported into Europe and North America and Canada, as well as those passing through the hands of the Hudson Bay Company. The European returns, owing to the abolition of duty, cease with the year 1844; but the other two sets of tables commence with the middle of last century and are continued down to 1890 or 1891. In the text further details are given as to the number of pelts of the various species which come into the market. These tables enable the naturalist to gain some idea of the appalling destruction of animal life which is continually going on for the purposes of commerce; while they likewise serve as an indication of the relative degree of abundance of any particular species, and also as to whether its numbers are being perceptibly diminished. In regard to the number of pelts furnished by certain animals, we may mention that in 1844 the common squirrel yielded no less than 2,289,838 skins; while in 1883 the Hudson Bay Company exported upwards of 1,082,999 skins of the Musquash, the total annual number of the pelts of that animal being estimated on page 261 at between two and three millions. Then, again, in 1889 the number of skins of the Australian Phalanger (Opossum) imported into the United States reached the enormous total of 3,169,571.

So far as we are able to judge, Mr. Poland has succeeded admirably in his task of collecting and correlating the returns of the number of skins of the various animals of which he treats; and from this point of view his book will always have a considerable value. Unfortunately, however, for his reputation, the author has thought it necessary to favour his readers with certain observations as to the

structure, habits, and distribution of the same animals, some of these observations being of the most startling nature.

With the present abundance of books on Natural History, and especially on mammals, we should have thought it would be possible for any writer who has no special knowledge of systematic zoology to give a fairly creditable account of the leading features of the animals he notices. Mr. Poland seems, however, to have carefully avoided reading all the more important recent works on mammals, and derives a considerable amount of such information as he possesses from the somewhat antiquated works of the Rev. J. G. Wood.

We notice, in the first place, that the author has a total lack of literary style, his sentences being short, disconnected, uncouth, and in some cases ungrammatical; while many of his remarks appear decidedly naïve. As an instance of grammar, we may quote the following sentence from page 304, where we read (the italics being our own) that "A few skins of the Harnessed Antelope are imported with monkey skins, and *they* are principally used for leather, although *it* would make up well for fur."

Not less glaring is the author's careless proof-reading of the scientific names of the various animals of which he treats—a matter which should have received the utmost attention in a work largely intended for readers who are not zoologists. As instances of this slipshod carelessness, we select the following, viz.:—(p. 11) *Semnopithecus schistanus*, for *schistaceus;* (p. 31) *Felis geoffroy*, instead of *geoffroyi;* (p. 32) *F. passerum*, for *pajeros;* (p. 53) *Cynæturus*, in lieu of *Cynælurus;* and (p. 59) *Nandinia binstata*, for *binotata*. Again, on p. 306, we have *Kerwas*, instead of *Kemas*, for the Tibetan Chiru Antelope, of which, by the way, the proper name is *Pantholops*.

With regard to the errors in zoology, the following are some of the most glaring. Thus on page 13 we find the astounding statement that all the Lemurs are confined to Madagascar; while on page 113 we are surprised to learn that in Northern India an "American species, the Racoon, is found." We presume the latter statement has originated from the name "Himalayan Racoon" having been applied to the Panda (*Ælurus fulgens*); but on turning to pp. 155,156, we find the author in hopeless confusion on this subject. We have, indeed, the Panda recorded on the latter of these pages under its proper title, without any reference to its affinity with the Racoons. On the preceding page we have, however, an animal mentioned under the name of the "Cashmere Racoon," with the sapient remark that "this extremely rare animal inhabits Cashmere, and the extreme North of India"; and it is added that "the *head* is ringed like the American Racoon." In the latter remark, the author makes the trifling error of writing *head* instead of *tail;* and, as a matter of fact, the animal to which he refers must be the Panda itself, which, by the way, is totally unknown in Kashmir, or anywhere else in the North-west Himalaya.

The author is almost equally at sea with regard to the Civets, which he appears to consider merely as "cats," since he commonly speaks of them by that name. First of all he alludes to the common Indian Civet (*Viverra zibetha*) under the title of the "Chinese Bush, or Civet Cat," and states that it is an inhabitant of China and Nepal, and is probably merely a variety of the African Civet (*V. civetta*). Subsequently, it is added that "a local variety is the Indian Civet *Viverricula malaccensis*." Now if the author had taken the trouble to consult Jerdan's or Blanford's works on the Mammals of India, he would have found that *V. zibetha* is to the full as distinctively an Indian as a

Chinese animal, its range extending from Bengal to South China; and he would not have written such arrant nonsense in regard to its relationship to *V. civetta*, or as to the very distinct *Viverricula malaccensis* being merely an Indian *variety* of this so-called Chinese species.

Mr. Poland seems, indeed, to be in a state of utter confusion as to what constitutes a species and what a variety, or, indeed, a genus. Thus, when treating, on pp. 170, 171 of the Indian Sloth-bear (*Melursus ursinus*), he observes that this "bear would probably interbreed with the Black Bear of America, and if the offspring of these two bears should prove fertile, it would necessitate their being classed as one species." Has the author, we may ask, ever seen these two animals, and does he know that they differ from one another more than any other two bears he could have mentioned,—the difference even extending to a disparity in the number of the teeth?

A still more appalling error confronts us on page 304, where the West African Zebra Antelope (*Cephalophus doria*) is alluded to as the "West African Striped Deer," without any technical name. We had thought that in these days even the proverbial schoolboy knew that there were no deer in Ethiopian Africa. As minor errors, we find the Beaver alluded to on page 247 as the largest existing rodent, while on page 245 the Red Himalayan Marmot (*Arctomys caudatus*) is said to be of a "deep grey hue," instead of yellowish tawny or orange, with a black back and tail. Again, on page 144 we find the Chinese Otter (whatever species that may be) alluded to by the name of *Lutra felina* (misprinted *felisa*), which properly designates a South American form.

In treating of the Kolinsky (*Mustela sibirica*), on page 114, Mr. Poland hurls an unjust accusation against English zoologists by observing that this animal "appears not to be mentioned in any work on Natural History in this country, and to be unknown to scientists with the exception of the Russian professors." If the author had turned to the late Dr. Gray's British Museum Catalogue of Carnivores, &c., published in 1869, he would have found the animal described on page 94; while in a recently-published work, entitled "The Study of Mammals," in which the author might possibly find some information that would be worth his attention, this animal is mentioned on page 586.

We might go further, but enough has been stated to show that, however valuable may be the author's information as to the particular subject of which the work treats, yet that his zoology must by no means be taken as gospel. In conclusion, we should like to ask Mr. Poland from what animal the figure given on page 127 as a representation of the Glutton was taken, and also how he acquired the information (p. 230) that the total number of Moles in Britain is between five and six millions.

R. L.

Les Mollusques: Introduction à l'étude de leur Organisation, Développement, Classification, Affinités et principaux Types. By Henri Coupin. For the use of Candidates for the Licentiateship in Natural Science. 8vo. Pp. 261. Paris: Carré, 1892.

The degree in Natural Science in the French universities would appear, according to our author's preface, to be more difficult of attainment than that in other faculties, and this inequality is to be attributed to the absence of suitable text-books treating of the subjects of examination. It is with the view of filling up one, at all

events, of these deficiencies that M. Coupin has prepared the treatise mentioned above. The work is intended for students preparing for examination, and from this standpoint it must, in fairness, be judged. We may say at once, then, that, though not perfect, it is much superior to the majority of works compiled with such an object in view.

Each class of the Mollusca is considered separately, and all generalisations referring to the group as a whole are deferred to the end of the work; a mode of treatment which has many advantages over the more usual plan of giving generalities first and details afterwards. As regards classification, we find six classes adopted:—*Acéphales, Scaphopodes, Amphineures, Gastéropodes, Ptéropodes, Céphalopodes.* No objection of weight can be urged against the use of *Acephala* for the first of these, though it is, perhaps, more suitable as applied to a dichotomous division of the Mollusca, and contrasted with *Cephalophora* or *Glossophora*. In his discussion of this group, the author departs from his usual practice of selecting a concrete example for description, giving only an account of an ideal type, with certain comparative observations. This seems to us an error of judgment; the study of an actual form impresses upon the mind of the student a quantity of definite information, and comparisons can be just as well instituted with a tangible example as with an ideal type.

With this exception, the treatment of the *Acephala* may be taken as representing that adopted in the case of the other classes, and it is on the whole satisfactory. The different systems of organs are taken up seriatim, beginning with the shell, mantle, muscles, and so forth. The descriptions are clear and concise, and are profusely illustrated by cuts, some copied from well-known works, but the greater number evidently, as stated by the author in his preface, reproductions of lecture diagrams. Many are rather rough, but they are clear, and a great help in the elucidation of the text. The work is well up to date, and in many cases references are given to original sources of information, though not so frequently as might be desirable. Following upon the anatomy are a few pages treating of embryology, then comes a *résumé* in half-a-dozen lines of the main characters of the group, and finally a systematic view of its principal families, with occasional mention of conspicuous genera. Some of these notes are good and to the point, but others are too brief to be of any use; as for instance, under "9° *Mactridæ*" the only information vouchsafed is "Les *Mactra* ont une coquille ventrue." Precisely the same character is emphasised in the case of the *Arcadæ*.

It seems needless to follow the author in detail through his treatment of the remaining groups. We cannot, however, conceal our astonishment that he should retain the Pteropoda as a distinct class, when he is evidently well acquainted with the work of Pelseneer and others on this question, and, by his own admission (p. 191), attaches great weight to their arguments.

The descriptions are in the main accurate, though errors in matters of detail occur here and there, as, for instance, when the figure of a tentacular club of *Sepia tuberculata* is made to do duty for that of *S. officinalis*, and no mention is made of the infrabuccal cushion on which the female *Sepia* receives the spermatophores. Taken all in all, however, the book is probably the best small text-book extant of malacology as opposed to conchology. It is much to be regretted that its pages are disfigured by a plentiful crop of misprints.

W. E. H.

AN INTRODUCTION TO THE STUDY OF THE GENERA OF PALÆOZOIC BRACHIOPODA. Part I. By James Hall, assisted by John M. Clarke. [Geological Survey of the State of New York, Palæontology, vol. viii., 1892.] Large 8vo. Pp. xvi., 367. Pls. 43. Price $5.

THE study of fossil Brachiopods, especially the Palæozoic, is beset with many difficulties. It has long been considered, and with much reason, that the only true basis of classification is to be found in the variations of internal structure, in the muscle markings, and in the calcified support of the lophophore or so-called "arms." Rarely, however, is a specimen found that shows or even has preserved this internal structure. Many a fossil must be ground down or dissected out with infinite labour, only to find nothing in the end. And when something has been discovered, and we are told, for instance, that a fossil long known as *Atrypa* is really a *Nucleospira*, still the geologist or the working palæontologist is not much better off; for the resemblances in outward shape between some species of widely-separated genera are often so great that we cannot, by ordinary methods, tell with which we have to deal.

For these reasons a hearty welcome must be extended to anyone that points out features of classificatory value hitherto unrecognised, or that applies to the study of the Brachiopoda principles that their students have up till now neglected. This has recently been done by Dr. C. E. Beecher, of New Haven, Conn., in some interesting and suggestive articles in the *American Journal of Science* (April, 1891, and August, 1892). He has been the first to regard the Brachiopoda, not as a number of separate species and genera that have somehow to be arranged in several families, but as a great series of living and growing individuals, one linked to the other and transmitting to it its own characters, though in a slightly modified form.

America certainly seems to be the country to which we must look for future advances in our knowledge of the Brachiopoda. That there is magnificent material has long been known, but hitherto we have had to trust too largely to the Cincinnati collectors for the interpretation of it. At last there is arising a school of palæontologists who look at fossils with the eyes of evolutionists and morphologists and not with those of species-mongers, and who do not despise the literature of the subject because it happens to have been published in a far country or written in a foreign tongue. There have been palæontologists in America, of course; among those dealing with Invertebrata, the names of Meek and Worthen and Shumard are bright examples. It is not the absence of good work that we have so often had to lament, as the presence of so much that is bad. In the domain of the Brachiopoda, at least, there will in future be scant excuse for bad work; for there has just appeared the first part of a book that, while gathering up the labours of the past, indicates clearly the direction of future toil.

This work, of which the title is given above, is the joint production of the veteran palæontologist of America and of one of the most promising members of the new school. Professor Hall also acknowledges his indebtedness to his private assistant, Charles Schuchert, who has already published some valuable systematic papers on American Brachiopods.

The part now issued (June, 1892) retains the Articulata and Inarticulata as convenient temporary divisions, and deals with all the latter, and with the *Orthis* and *Strophomena* groups of the former. It is an interesting feature of the work that no attempt has been made

to split up the included genera into families; they are, however, arranged in such an order that their affinities are clearly seen, and so that one genus naturally leads on to the next. To touch on minor points, we rejoice to see that the writings of Pander at last receive proper recognition, a result of which is, that the name *Clitambonites* resumes the place long usurped by the *Orthisina* of D'Orbigny, a restoration that has, we believe, already been made in our own Natural History Museum. The much-abused name *Strophomena* is at last restricted to what has long been recognised as the type of Rafinesque's genus, viz., "*Leptæna planumbona*" of Hall. The well-known *Strophomena rhomboidalis* is naturally restored to its proper place in the *Leptæna* of Dalman (whose original plates, by the way, are here reproduced to the great advantage of American readers); while the forms usually called *Leptæna* by modern writers take their true name of *Plectambonites*, Pander.

These changes, which will, of course, appal a good many people, will be welcomed by all who have to work on the Brachiopoda, as not only doing justice to the older writers, but as bringing the matter out of the confusion into which it had been cast by the arbitrary restrictions and alterations of those who followed them. After all, attempts at compromise in these questions generally work more harm than good, and it is best in the end to follow the simple Law of Priority. We are a little apt to think too much of ourselves and of the momentary inconvenience that results, the inconvenience of re-writing a few labels or re-arranging a few specimens; and we do not think enough of those who are coming after us, and who are the inheritors of our weak-minded muddle-headedness no less than of any little advance in knowledge we may be fortunate enough to make.

F. A. B.

OUR TEMPERAMENTS: THEIR STUDY AND THEIR TEACHING. A Popular Outline. By Alexander Stewart, F.R.C.S., Edin. Second edition, carefully revised; indexes and additional illustrations, including chromolithographs from drawings by Lockhart Bogle. 8vo. Pp. xxviii., 412. London: Crosby, Lockwood and Son, 1892.

IT should be generally known—though it does not appear to be—that there are four main types of physical organisation to which, or to compounds of which, every man may be referred. These are the Temperaments, and they are named, in accordance with the system most prominent in each one, the Sanguine, the Bilious, the Lymphatic, and the Nervous, which last has replaced the old Melancholic. Medical men, from Hippocrates to Sir J. Crichton Browne, have recognised not only the existence of temperaments, but the importance of their study; and yet the physical and mental characteristics of each temperament are still far from known. Shakespeare and Addison remind us that the theory has been appreciated by poets and prose-writers of all time; and yet the word Temperament is, unfortunately, misused in our daily conversation and our daily Press. These thoughts have urged Mr. Stewart to publish the work of which a second edition lies before us.

Were the temperaments nothing more than expressions of outward characters, such as the red hair, blue eyes, florid complexion, and thick-set build that make up the sanguine temperament, their study would be little more than learned trifling. It is the theory that these outward marks are the signs of certain predicable mental

characters and of a tendency to certain diseases that invests the temperaments with their importance. Some such belief influences us all in our dealings with our fellow-men; the stage faithfully mirrors it in its tradition that the hero must be of sanguine, the villain of bilious temperament (the low comedy is generally lymphatic). Stage-types, however, are rare in real life, and it is the prevalence of organisations in which the elements (to use Shakespeare's phrase) are mixed, that is, of compound temperaments, which raises difficulties to the practical application of the theory. It is on the discrimination of these mixed types that Mr. Stewart lays particular stress. He claims to have rendered the study more easy by the ascription of form, as well as colour-characters, to the chief temperaments. Since, however, the form-characters of the sanguine, bilious, and lymphatic temperament are all the same, it does not appear that this indicates any great advance.

Still, as of old time, the great battle is being fought between matter and spirit. Is it really true that every mental and moral trait of an individual is indissolubly bound up with the structure of his flesh and blood? Nay, more, that the connection is so great that from the outer we could, with but a little more knowledge, predicate the inner man? Does a man enter on life with ready-made works, a given motion, and a spring that will work so long and no longer—the only thing that can modify his action mere external circumstances? These are questions of supreme importance, but they will not be answered by vague generalities. The great want in Mr. Stewart's book consists in facts. A few are quoted from Mr. Francis Galton and Dr. Beddoe; but beyond the author's own statement, we can find no indication that he has studied the subject in a scientific manner.

The book, however, is an interesting compilation and contains some sensible, if rather obvious, remarks on education, the choice of a profession, and the duty of biographers. *Apropos* of the remarks on the last subject, we may complain that Mr. Stewart does not tell us what his own temperament is. We infer, however, that he is sanguine, since he seems to have the following mental characteristics:—"Favourable conclusions thoughtlessly drawn," "Equally happy in the pursuit of little as of great ends," "Not minutely informed."

WOODWORK. By S. Barter. Pp. 343. London: Whittaker & Co., 1892. Price 7s. 6d.
MANUAL OF INSTRUCTION IN WOODWORK. By George Wood. Pp. 104. Leeds: E. J. Arnold, 1892. Price 5s.

CIRCULAR 44, issued by the Science and Art Department in 1890, contains several suggestions respecting the course of manual instruction in elementary schools. Paragraph 5 suggests that "clear ideas respecting the growth and structure of woods may be given by means of a small collection of the different descriptions of timber used in carpentry." Writers of books on Manual Training in Woodwork, with this suggestion in view, have thus included one or more chapters dealing with the purely scientific aspect of the subject, but it is to be regretted that authors capable of giving valuable information on the best methods to adopt in manual training classes did not secure some competent co-worker to write the strictly botanical part of the book. If it is necessary to carry out all the Department's suggestions in one book, there is a clear case for collaboration. The astounding

errors into which the writers have fallen make it quite necessary to warn teachers about to consult books on manual instruction that they should read the chapters on the growth and structure of trees with much caution, or, better still, they should not read them at all. The information which they require can best be obtained from a good text-book on elementary botany, or from that capital book "The Oak," by Marshall Ward.

In illustration of our contention, we have selected the two recently published works mentioned above. In each of these books we find whole paragraphs incorrect in every detail, the writers having anything but "clear ideas" of how a tree really grows, and what is the true nature of the current of water which passes from the roots upwards. The transpiration current, as such, is not mentioned, and the true function of foliage-leaves, together with the distribution of the plastic substances to all growing organs, is totally ignored. The result of this year's examinations having just been published, it would be interesting to know how many teachers who used these books failed to express themselves clearly in answering questions on the mode of growth of plants.

In the first-mentioned work we note, on page 40: "A slowly-flowing current of sap, obtained principally by the roots, ascends the tree, and the wood-forming materials contained in it feed and thicken the walls of the tracheides." On the subject of felling we are told: "A tree should always be felled when the sap is at a standstill—the height of summer—when it has ceased to run up the tree, but has not yet begun to return; or the winter, when it has completely run down" (page 44).

From this the teacher of manual training is to obtain "clear ideas," which he will hand on to his class. It is no great stretch of the imagination to picture a boy going home from his lesson with a confused notion that sap has a kind of clockwork action; it occasionally "runs down," and sometimes "stops."

The following, from the second work, reads like a huge joke: "The rising or common sap containing much moisture flows towards the leaves. Oxygen and moisture are evaporated, while carbonic acid is absorbed by means of the stomata. The sap, now changed to proper sap, descends in the autumn towards the roots between the bark and the stem, depositing new woody fibres in the cambium (sticky substance) surrounding the stem. Thus, a new deposit of woody matter is formed from the sap all round the stem."

It is rather startling to be gravely told that the sap contains much moisture, and that the proper sap descends in autumn after staying up aloft for so long!

We trust that should a second edition of either of these handbooks be called for (they are capital guides respecting the manual instruction) the writers will see that the part dealing with the growth and structure of trees is entirely re-written by persons competent to deal with the subject.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

DR. R. VON LENDENFELD has been appointed Professor of Zoology at Czernowitz, Galicia.

DR. HENRY B. WARD has been appointed Instructor in Invertebrate Morphology in the University of Michigan, Ann Arbor; and Dr. Charles L. Edwards directs the Biological Institute of the University of Texas at Austin.

THE opening address of the present session at the Owens College, Manchester, will be delivered early this month by Mr. F. E. Weiss, the newly-elected Professor of Botany.

THE Annual Report of the British Museum for 1891 has been received. The Statement of Progress in the Departments of Natural History will be read with much satisfaction, and the list of publications issued during the year includes eight volumes of the well-known Catalogues. The number of acquisitions is especially large, including no less than 1,186 presents, and several of these are of great value. The Natural History collection bequeathed to the Trustees by the late Dr. R. M'Cormick, includes many important specimens from the Arctic and Antarctic Regions; and a large collection of Zoological specimens made by Mr. J. J. Walker in the seas off the W. and N.W. coasts of Australia adds many valuable marine organisms. A fine series of nearly 290 horns and heads of Indian mammals has been presented by Mr. Allan O. Hume, C.B.; and another large collection of skins and spirit specimens of mammals from the same country is the gift of Dr. W. T. Blanford, F.R.S. Among beetles, there is another donation from Messrs. Salvin and Godman; and among moths the chief present is a series of Ceylonese specimens, selected from the cabinet of Mr. E. E. Green. The largest donation of fossils is that of Mr. S. J. Hawkins, comprising valuable specimens from the Chalk of Kent, and mention ought also to be made of a unique portion of skull of the Saiga Antelope from Thames gravel at Twickenham, discovered and presented by Dr. J. R. Leeson.

The Director records the extension of the building for the accommodation of specimens preserved in spirit, and especially points out the desirability of increasing this collection. Greater attention is now paid to anatomical characters, as indicating the true position and affinities of animals, and these are often not exhibited in the dried specimens which formerly sufficed for the needs of the zoologist, but can only be studied in specimens preserved in alcohol.

THOSE who are interested in the British Museum more as a place of education than a store-house, will observe with especial pleasure the report of last year's course of lectures on Geology delivered by Professor Alleyne Nicholson on the Swiney Foundation. The attendance of the public was far larger than formerly, the average number being 214, and on several occasions the late-comers could not be admitted from want of accommodation. The director remarks that if the atten-

dance is maintained in future years, it will be necessary for the trustees to consider the question of providing a properly-constructed lecture-theatre. Professor Nicholson's course this year begins on Monday, October 3, at 3 p.m., and will be continued on each Monday, Wednesday, and Friday for one month. The subject is, "The Great Periods of Geological History," and a detailed syllabus may be obtained from the director of the British Museum (Natural History), South Kensington.

WE learn that the collection formed by Mr. Hose in Borneo has been acquired by the British Museum. This collection is rich in birds, and will, it is hoped, yield many forms previously undescribed or of special interest.

THE Palm House at Kew has lately been enriched by a healthy young specimen of the Palmyra Palm (*Borassus flabelliformis*), a present from the Gaekwar of Baroda. It has a trunk 2 feet in diameter and 4½ feet in length. Attempts to grow this palm to any size at Kew have hitherto failed, though at present there are some promising seedlings raised from seeds presented by Sir M. E. Grant-Duff in 1886. The rich collection of Filmy Ferns has just been arranged in a new house measuring 50 feet by 14, with a central path and two cases running the full length. The Museum has also been enriched by a present from Mr. Charles Holme of an extensive valuable series of products illustrating the uses of the Bamboo in Japan. It gives a good idea of the great value of this tree-like grass, and the ingenuity of the people in adapting it.

THE recent disastrous hurricane at Mauritius almost completely destroyed the beautiful Botanic Gardens. The *Kew Bulletin* quotes from a letter of Mr. W. Scott, the Acting-Director, who says:—"The sugar-canes have been levelled, and their leaves threshed into fibre. The gardens at Pamplemousses are a complete wreck; the oldest and finest trees have been uprooted, and the trees that are still standing are reduced to bare poles. The fruit trees have been so smashed up that I can hardly recognise them. In some parts the wreckage is piled up nine feet high, and it will be months before we can get it cleared away."

THE British Institute of Public Health held its annual Congress in Dublin this year, under the presidency of Sir Chas. Cameron, on the 17th August and following days. The first two days were chiefly occupied with the reading of papers, and the concluding days with excursions. Some papers on bacteriological subjects were of interest to naturalists. Dr. M. Weeney, of the Mater Misericordiæ Hospital, Dublin (which posseses the only bacteriological laboratory in Ireland), dealt with the question as to which *Bacillus*, *B. typhous*, or *B. coli-communis*, causes typhoid fever. Mr. W. E. Adeney, in a paper on the Chemical Bacteriology of Sewage, attributed the oxidation of waste organic matter to the life-processes of certain micro-organisms. The most important of the excursions was that to the artificial lake among the Wicklow Hills, where the waters of the Vartry are impounded to form a supply for the City of Dublin.

THE fifteenth annual meeting of the Midland Union of Natural History Societies is reported in the *Midland Naturalist* for September. It was held at Oswestry, on August 24th and 25th, under the presidency of Mr. A. T. Jebb, of Ellesmere, and there was a good attendance. The report of the Council makes special reference to the function of the Union as an organiser of scientific work—"a central organisation, by means of which the scientific work of the more active societies in the Midlands may be rendered more effective"—and then laments the meagre character of the results with which it has to deal. In Botany, indeed, the Executive Committee reports that no completed paper or papers of a character to warrant the bestowal of the Darwin medal have appeared in the *Midland Naturalist* during the past three years, and the award is thus not made this year. The subject

for next year's award is that of Geology. During the last session the Cheltenham Natural Science Society and the Worcestershire Naturalists' Field Club have joined the Union, while the Tamworth Natural History and Antiquarian Society has ceased to exist.

AT the twenty-first annual Conversazione of the Chester Society of Natural Science held last month, the Kingsley Memorial Medal was awarded to Mr. Osmund W. Jeffs, the energetic secretary of the British Association Committee on Geological Photographs.

SEVERAL new publications of the British Field Clubs have lately been issued. Vol. vi. of the *Transactions of the Hertfordshire Natural History Society and Field Club* is now completed by the issue of parts 8 and 9, which comprise a record of proceedings and excursions, with an index and list of members. The *Essex Naturalist* continues actively to record the fauna and flora of the county to which it relates. Few of the papers are of more than local interest, but the proximity of Epping Forest and of the Essex marshes will make the journal interesting to the London naturalist. We are sorry to read so many records of the extermination of rare species.

THE *Proceedings and Transactions of the Croydon Microscopical and Natural History Club*, Feb. 11, 1891—Jan. 13, 1892, contain little original matter beyond tables of rainfall. The reports of the six sub-committees (or sections) occupy only four pages, and among the chief achievements of the geologists during the year it is recorded that photographs were taken "of an interesting old gabled farmhouse." The Malden Natural History Society issues a small 4-page monthly *Gazette*, price one penny, with very brief general articles. Part I. of the new volume (xxiv.) of the *Transactions of the Cardiff Naturalists' Society* contains a readable report on Meteorology, by F. G. Evans; an article on Hardy Fruit Culture in South Wales, by A. Pettigrew; a description of the Neath Valley, by T. H. Thomas, and smaller notes.

THE *Transactions of the Norfolk and Norwich Naturalists' Society* for 1891–92 contain several papers of more than local interest, though as usual their strong point is local natural history. The President, Mr. F. D. Wheeler, writes of the extinction of the Fenland lepidoptera; Dr. C. B. Plowright on Neolithic implements in West Norfolk; Mr. Southwell on the St. Helen's Swan-pit (near Norwich); Mr. Clement Reid on the Natural History of Isolated Ponds; Mr. Aplin on the distribution of the Red-backed Shrike; and we have the usual miscellaneous notes on local meteorology, ornithology, and fishes. Why does not this society do anything with the marine invertebrata? Norfolk has an extensive coast-line.

THE *Report and Transactions of the Penzance Natural History and Antiquarian Society* for 1891–92 completes vol. iii. of the new series. The Council suggests the award of prizes to juveniles for collections of specimens by way of fostering an interest in Natural History, the society being much in want of members and contributors to its museum. The Transactions deal chiefly with antiquities, but there are also valuable lists of sea-anemones, corals, beetles, and mosses.

OBSERVATIONS AND CORRESPONDENCE.

Professor Giard on the Evolution of Flat-Fishes.

Professor Giard replies to my article on "The Evolution of Flat-Fishes" as if I had commenced the controversy between us by making an attack on him. The fact is, on the contrary, that my criticism of his note on an abnormal turbot was a defence of my experiments as described in the *Zoologischer Anzeiger* against his attempt to prove that they were superfluous on the one hand and inconclusive on the other. The Professor deliberately maintained that the existence of specimens of *Rhombus maximus* in which the head was in certain respects monstrous or abnormal, and in which the blind side was pigmented all over like the eyed side, was in itself a sufficient proof that the coloration of the upper sides of flat-fishes was due to the direct action of light, and that pigment was produced on the lower sides of such fishes when those sides were exposed to light. My opponent still refuses to admit the cogency of my arguments and the validity of my defence, and I will try, therefore, to answer his objections.

Professor Giard distinguishes between what he calls monstrous Pleuronectids which show arrested development and the double flat-fishes whose existence he says I stated to be recorded in numerous works on Ichthyology. The distinction is valid, and I have never ignored it: there are ambicolorate specimens which are normal in all respects except colour, and there are ambicolorate specimens in which the head and eyes have the abnormality described in Giard's specimen. But what I said was, that the "monstrous Pleuronectids which show arrested development without stoppage in growth," to use the Professor's words, and not merely ambicolorate specimens, were recorded and described in a large number of ichthyological treatises, and that the specimens recorded did not belong exclusively to the one species *Rhombus maximus*, as Professor Giard stated, but to other species and other genera as well. So much he now admits, citing numerous cases in literature. He refers to a specimen of the turbot described by Couch, and quotes that author as stating that the same monstrosity is not very rare in *Zeugopterus punctatus*. I regret that he has not given volume and page for these references to Couch, for in Couch's general work on "Fishes of the British Islands," vol. iii., I find a description and figure of the monstrosity in question in the flounder, *Pleuronectes flesus*, but no mention of it in connection with *Rhombus* or *Zeugopterus*.

But in correcting his original statement Professor Giard says the monstrosity occurs among the flounders, "whose development is as slow as that of the turbot," and that it may be stated in a general way to occur among the Pleuronectidæ of gradual metamorphosis (palingenetic development). I confess I cannot fully understand these statements, but so far as they convey a meaning to me I entirely disagree with them. The exact age of the turbot and flounder at the beginning and end of metamorphosis, and the exact time occupied in that process, has never, so far as I know, been ascertained. But it is certain that the development of the turbot and brill is in some respects peculiar and different not only from that of the flounder, but from that of all other Pleuronectidæ. The two species of *Rhombus* have a well-developed air-bladder during their metamorphosis, and, in consequence of this, swim near the surface of the sea until the metamorphosis is complete, or nearly so; they also attain an unusually large size before they acquire the adult characters. I have specimens of the turbot 3·6 cm. in length in which the transformation is by no means finished. The flounder, on the other hand, and all species of the genus *Pleuronectes* have no air-bladder during metamorphosis, and begin to lie on the bottom at the very commencement of that process. The flounder has acquired the characters of the adult

when it is 2 cm. in length, and yet it is larger, when first hatched, than the newly-hatched turbot. All other genera whose larval stages have been described resemble *Pleuronectes* in their development and differ from *Rhombus*, excluding, of course, the so-called *Plagusia*, which has a peculiarity of its own. I cannot understand, therefore, what is the similarity in development between turbot and flounder to which Professor Giard alludes. I equally fail to understand how any Pleuronectidæ can be distinguished from the rest as having a gradual metamorphosis or palingenetic development, since in that respect they all agree. Every Pleuronectid is hatched as a perfectly symmetrical larva swimming vertically in the sea, and only acquires the peculiarities of the adult after a gradual metamorphosis, in which one eye passes from its original position to a new position on the opposite side of the head.

If Professor Giard has discovered a true Pleuronectid which is hatched in the asymmetrical condition, he should say so; and I should be the first to admit that he had made a most remarkable and interesting discovery. It is, of course, true that there is another mode of metamorphosis among Pleuronectidæ besides that which occurs in the majority of genera such as *Rhombus* and *Pleuronectes*. This other mode is that described by Steenstrup and Agassiz in *Plagusia*, in which genus the eye passes through the base of the dorsal fin to reach the opposite side of the head. But surely the Professor would not assert that this mode is less palingenetic than the other? It is possible, indeed, that the early development of the dorsal fin is a cenogenetic character, but the phylogenetic history is repeated in the bilaterally symmetrical larva and the gradual torsion of the eyes in *Plagusia* as in other Pleuronectidæ.

I am not concerned to reply to Professor Giard's remarks about the abundance of ambicolorate specimens in which no other abnormality is present. I need only say that I am perfectly aware of their comparative commonness, and the point is not in dispute between us.

We come now to Professor Giard's reply to my criticism of his assumption that the abnormal turbot he described must have swum in a vertical position.

Neither in his original note nor in his reply to my article has the Professor asserted that he ever saw an abnormal turbot, of adult size, swimming vertically in the water; he has simply said that his specimen *devoit nager* in that position. He asserts he had the testimony of Day and McIntosh in support of that opinion, but this statement is not strictly accurate. As I stated in my previous article, I have failed to find a particle of direct evidence, a single recorded observation, to prove that abnormal flat-fishes swim vertically. Day (*loc. cit.*) says that double flat-fishes "are seen to swim vertically and to be more frequently found near the surface of the water than those which progress in a normal manner." But he does not say he has seen this himself, nor mention anyone who has seen it. As for Professor McIntosh, he has not in either of the works cited by Professor Giard, namely, "Marine Invertebrates and Fishes of St. Andrew's," and the memoir on the "Development of Teleostean Fishes," made any statement at all in reference to the position in swimming of monstrous adult specimens of flat-fishes of the same kind as Professor Giard's turbot. I am considering now abnormal monstrous specimens of adult size, not larvæ at a stage of metamorphosis, which are, of course, normal. As there are no recorded observations as to the mode of swimming of the monstrous adult specimens, certainly none cited by Professor Giard, I could not, and did not, assert that such observations were erroneous.

The Professor's method of controversy seems to me lamentably disingenuous. He assumes that his monstrous turbot swam vertically, and I pointed out that there was no evidence to prove this; yet he now maintains that he had the testimony of Day and McIntosh to rely upon, although, as I have shown above, neither of those authorities gives any evidence. My remark, "this is an error of observation," referred to a perfectly distinct and separate question, namely, the position, in swimming, of *normal larval* specimens whose metamorphosis is in normal progress. Professor Giard, in his original note, stated that Professor McIntosh had described "un individu normal de 9 millimètres de long sur 7 millimètres de large, dont l'œil droit était déjà arrivé sur la crête du dos, et qui nageait encore dans la position verticale." I replied that the assertion of its swimming in a vertical position was

due to an error of observation, basing this reply not, as the Professor says, on the observation of an adult double plaice, which has nothing to do with the case of a larva, but on the repeated observation of dozens of larval turbot at the stage of metamorphosis described by Professor McIntosh. But I find, on referring to the passage in question (*Trans. Roy. Soc. Edinb.*, vol. xxxv., pt. iii., p. 846), that I owe an apology to Professor McIntosh. The error is not an error of observation on his part, but an error of citation on the part of Professor Giard. Professor McIntosh does not state that the larval specimen in question swam constantly in the vertical position. He does not state that he saw it alive. All he says in reference to its position is as follows: "The appearance of certain spines on both sides after the right eye is at the edge, indicates the possibility that for some time the fish may *occasionally resume* the vertical position in swimming."

I must mention here, lest I should be suspected of omitting anything, that the only other citation of Professor McIntosh by Professor Giard, namely, from the "Marine Invertebrata, etc., of St. Andrew's," refers to a young turbot which had an eye on each side, and in which the dorsal fin did not form any hooked process, and which was, therefore, quite different from the typical form of arrested development under consideration.

Professor Giard states that the double plaice which I described in my article as being under my observation in captivity was not monstrous and only imperfectly double. I stated that it *was* monstrous in Professor Giard's sense: that the right eye was on the edge of the head and the dorsal fin terminated behind that eye, though it does not form so prominent a hook as in other cases. What excuse is there to be made for the Professor when he thus asserts that my specimen has the head and eyes normal, although I expressly described the head and eyes as abnormal?

Professor Giard now admits that ambicolorate specimens with normal eyes lie horizontally on the ground. That being so, how can an ambicolorate specimen, with an abnormal head, be a proof of the direct action of the light in producing pigment on the side where it is normally absent?

Professor Giard now affirms not that the monstrous specimens swim all their life vertically, but that they remain longer in that position than others, in any case long enough to allow the influence of the light to act efficaciously on the side ordinarily colourless. But he has less evidence, if possible, to support this than he had to support his original assumption. My contention has been throughout that evidence is required on the question of the relation of pigmentation to light. I produced some evidence in the published result of my experiment. Professor Giard has failed to appreciate that evidence; he has produced no evidence himself, but, while recording a specimen of a familiar abnormality, has attempted to maintain that the existence of this abnormality renders my evidence superfluous. He accuses me of an astonishing misconception of the laws of cross-breeding. The misconception was not mine, for I simply pointed out the necessary consequences of the action of inheritance on the colour of flounders as conceived by himself. Supposing we accept his correction, what is the application to the results of my experiment? The progeny cannot be intermediate, he says, between two progenitors, but must resemble closely one or the other. Therefore, the progeny of a right-sided and a left-sided flounder must be either right-sided or left-sided. But the flounders in my experiment were coloured on both sides, and, therefore, the possibility of the occurrence of reversed examples among their ancestors had nothing whatever to do with their condition, as Giard originally suggested.

This controversy was not of my seeking, and I can only regret that Professor Giard should have been betrayed in discussing this subject into a temporary departure from the accuracy and caution which he usually exhibits. Any zoologist who reads the literature must admit that satisfactory evidence as to the causes which produce abnormalities of colour and structure in flat-fishes is not at present available. That the typical ambicolorate monstrosity in which the metamorphosis of the head is permanently arrested is produced by the action of light, or by the direct action of conditions at all, is a pure assumption unsupported by any evidence, and, therefore, to maintain that the occurrence of such specimens is a proof that pigmentation of the skin is dependent upon light, or its absence on the absence of light,

is simply to ignore the nature of proof. All that I have ventured to maintain is that, under the conditions of my experiments, the action of light determined the production of pigment on the lower side of flounders. It by no means follows that the occurrence of pigment on the lower side of a flat-fish under natural conditions has been due to the direct action of light.

In conclusion, I will add a few words of explanation which will, perhaps, enable Professor Giard to understand why I felt it necessary to criticise his note so severely. Like himself, I have a strong leaning towards Lamarckian principles, and I considered that the almost universal fact that pigment is more abundant on the dorsal than on the ventral surfaces of animals, and the fact that whereas in fishes generally the ventral surface, which is turned downwards, is pigmentless in the flatshes, the whole of the lower side has become pigmentless and the whole of the upper uniformly pigmented, indicated strongly that the incidence of light rays on the skin caused the deposition of pigment. I studied the question of ambicolorate specimens and could find no sufficient evidence that in them the lower sides had been more exposed to light than normal specimens, and I felt that sufficient evidence in a case of this kind could never be got by observation of fishes in their natural wild state. I came to the conclusion that questions of this kind could only be decided by experiment. Having succeeded beyond my expectation, I was naturally indignant when a zoologist of reputation, from whom I had reason to expect sympathy and support, should come forward and publicly depreciate my methods and my results, and endeavour to throw the whole subject into the hopeless quagmire of fallacy which I was trying so carefully to avoid. J. T. CUNNINGHAM.

PROFESSOR MIVART ON THE FOUNDATIONS OF SCIENCE.

THE *ultimate* basis of all science (and it is an ultimate basis which forms the object of Professor Mivart's discussion) can be nothing less than the general conditions of consciousness upon which the very possibility of experience depends. It is possible to enunciate a large number of propositions, psychological, logical, or otherwise, which *may* be self-evident and which *may* be necessary, but which may yet have no claim to be the foundations of science. Furthermore, the validity of many such propositions remains doubtful so long as their connections with principles more fundamental are still to seek.

Professor Mivart's "Intellectualism" seems to be in this predicament; for as a system it begs the whole question of the theory of cognition. Thus, of the four "essential truths" each of the first three lays down as a postulate without proof or definition (and so presumably as ultimate) what is in truth the very problem to be solved. The first talks of *substantial* and *continuous* existence, and the second of *real* existence, without any suggestion of a suspicion that these words raise the whole question as to what is the nature of human knowledge; while the third even more naïvely asserts that there are truths which are apart from the existence of any or every mind and that *this* is matter of knowledge.

As an example to prove that we have knowledge which is not conditioned by our faculties, Professor Mivart instances the case of number. But surely it may be questioned whether "facts concerning number," to use Professor Mivart's phrase, would exist at all if it were not for our faculties of perception. Thanks to our faculties of perception we have objects to count, and if intuition did not give us objects to count, can we suppose that numbers would have for us any significance whatever?

The objection which Professor Mivart makes to the Kantian view of Space and Time (which, indeed, has often been made and refuted before) does not seem to me serious, because it is really irrelevant. Space and Time certainly do take their place as abstractions whenever in the course of our studies we make *them* the objects of empirical thought. As abstractions, they then come before us among other contents of consciousness. But the fact that we can or must think of them thus when we treat them as objects has nothing to do with the question as to whether they have a just claim to be regarded as fundamental conditions of the production of experience by sensuous intuition.

The "other truth" which Professor Mivart puts forward as of special impor-

tance for Biology, justifies even more forcibly the charge which has in the preceding remarks been made against *Intellectualism*, that it makes no attempt to face the really fundamental problems for which critical Idealism has, whether successfully or not, at least offered us its solution. We may *talk* of "a principle which is neither a material substance nor a physical force," but philosophy has yet to begin *its* work. Surely we have a right to ask what do you mean by material and immaterial? What do you mean by substance? What is the source of these conceptions? What is their content? What is their validity? And so long as such questions remain unanswered we may well doubt whether the system of Intellectualism does more than offer doubtful dogmas in the place of firm foundations for the sciences.

R. J. RYLE.

THE GRAMMAR OF SCIENCE.

ALLOW me to thank Dr. Ryle for drawing attention to a misprint or slip of the pen in my Review (NAT. SCI., vol. i., p. 305) of Professor Pearson's *Grammar of Science*. "Perceptual conception" is obviously self-contradictory. What I wrote, or intended to write, was "perceptual surface."

Allow me also to assure Dr. Ryle that (as those who have done me the honour to read what I have written thereon are in a position to know) I do trust implicitly, and without fear or hesitation, in the results of psychological analysis; that I do not wish to "shelve" (in the sense of neglecting) either physics or psychology, though I do advocate our keeping them on different shelves; that it is not I who forget "that human experience is none the less real . . . . if it can be shown that the terms which express that experience owe all their significance to their inseparable association with the nature of our perceptive faculty"; and that, if I do serve two masters, I have at least the wit to know what service is due to each.

C. LLOYD MORGAN.

THE PERMANENCE OF OCEANS AND CONTINENTS.

IN Dr. Wallace's paper on "The Permanence of the Great Ocean Basins," published in the August number of NATURAL SCIENCE, p. 418, he mentioned as a reason for bringing forward new arguments in favour of the theory of permanence the circumstance that that theory had been attacked by Mr. Jukes-Browne in "The Building of the British Isles," and by myself in an address to the Geological Society, in 1890. Mr. Jukes-Browne has very justly shown, in his paper on "The Evolution of Oceans and Continents," printed in the September number of this journal, that Dr. Wallace's arguments are directed against views not supported by either of us and now held by but very few, if by any, well-informed geologists. How far such extreme views were advocated in my address, and in what respects that address should be regarded as an attack on the permanence theory, will, I think, be shown by the following extract, taken from the concluding remarks (*Proc. Geol. Soc.*, 1890, p. 107):—

"It will thus be seen that while the general permanence of ocean-basins and continental areas cannot be said to be established on anything like firm proof, the general evidence in favour of this view is very strong. But there is no evidence whatever in favour of the extreme view accepted by some physicists and geologists that every ocean-bed now more than 1,000 fathoms deep has always been ocean, and that no part of the continental area has ever been beneath the deep sea."

I think anyone who reads my address will see that this passage is a fair summary, and that my principal arguments applied, not to the theory of general permanence, but to the view then held by Dr. Wallace and some other naturalists and geologists, that the continental area is limited by the 1,000-fathom line. This view is now admitted to be untenable by Dr. Wallace, and as he concurs in the possibility of ancient land-connections between the three southern continents and the Antarctic land, he concedes every case that I dwelt upon except one, the existence, in Mesozoic times, of land uniting Madagascar and India, to which he does not refer. Under these circumstances, I think anyone reading Dr. Wallace's paper might suppose that the differences between us are much greater than is really the case.

The three arguments now brought forward by Dr. Wallace, are, as he says, "altogether inconsistent with any general interchange of oceanic and continental

areas"; but the first does not apply to partial and local changes; the second, as Mr. Jukes-Browne has shown, is open to considerable question; and the third, which appears to me of more weight, is an old argument restated, for it depends entirely upon the absence of deep-sea formations in continental rocks. This I have always regarded as the strongest and most important piece of evidence in favour of the permanence of continental areas. I dealt with the subject at some length in the address already quoted, and I can only add that the caution I then recommended in accepting the evidence as complete, and in supposing that no deep-sea deposits exist in continental areas where none have hitherto been detected, has been justified by several additional discoveries of such deposits in various parts of the world.

Professor James Geikie, in the very interesting address recently delivered to the Geographical Section of the British Association at Edinburgh, expressed himself thus:—"The continental plateau and the oceanic hollows have never changed places, although from time to time portions of the latter have been ridged up and added to the margins of the former, while ever and anon marginal portions of the plateau have sunk down to very considerable depths." This appears to me fairly to represent our present knowledge of the subject, and to be in no respect opposed to the views I expressed in 1890.

W. T. BLANFORD.

THE DISPERSAL OF FOREST TREES.

I WOULD ask Naturalists to study during the present month the dispersal of acorns. Seedling oaks are found abundantly in peat-mosses, and on open heaths at considerable distances from any mature trees. The dispersal of the acorns is generally, and I believe correctly, referred to the agency of rooks, which at this season desert the open fields for the oak copses. Actual observations are wanted as to how far a rook will carry an uninjured acorn in its beak, and whether the carrying of the acorn into the open to eat is a habitual action, or only done occasionally. One constantly finds torn (not gnawed) acorn-husks and half-eaten (pecked) acorns in open fields.

The question involved is a wider one than the mere inquiry: Why do seedling oaks appear in such unexpected places? Taking the oak as a type of our forest trees, we would inquire: How fast can a forest spread after a climatic or other change has rendered the country habitable? And also: Can large-fruited trees, like the oak, cross a strait or arm of the sea? The oak has a large, soft fruit; if fruit of this character can be carried considerable distances by birds, then it may be possible to account for the whole of our existing flora, without any land-connection with the continent since the passing away of the cold of the Glacial Epoch.

CLEMENT REID.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*

DR. HENRY MCCOOK (3100, *Chestnut Street, Philadelphia, U.S.A.*) desires the address of Mr. Stillbers, of London, who is reported to have successfully reared spiders for purposes of silk-culture. His address or references to any published account of his experiments will greatly oblige.

ERRATA.

OWING to the negligence of a clerk, the corrected proofs of Messrs. Jukes-Browne and Cunningham's contributions last month were overlooked. The two last paragraphs in Mr. Jukes-Browne's article on Oceans and Continents were transposed; and Mr. Cunningham is not responsible for the erroneous statement (in his review of Romanes' "Before and After Darwin," p. 544) that "Haeckel never used the name *Prophysema*." The Editor regrets the mishap and tenders apologies.